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behalf of the International Commission on Zoological
Nomenclature
has great pleasure in expressing its grateful
thanks to the
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the present volume

FOREWORD

by

The Lord Hurcomb,

Chairman of the
International Trust for Zoological Nomenclature

The present is the second of the two volumes of the *Bulletin of Zoological Nomenclature* which in the autumn of last year were allocated by the International Trust for Zoological Nomenclature for the publication of documents submitted for inclusion in the Zoological Nomenclature Agenda Paper for the Fifteenth International Congress of Zoology at its Meeting to be held in London in July 1958. The documents reserved for publication in this volume were of two kinds :—(1) documents containing suggestions for the further amendment of the *Règles* as approved by the Paris and Copenhagen Congresses of 1948 and 1953 respectively ; (2) proposals for the amendment of the Draft of the *Règles* as approved by the above Congresses prepared by Professor J. Chester Bradley, President of the International Commission on Zoological Nomenclature.

The number of documents falling in the above classes greatly exceeded expectations with the result that instead of containing between 500 and 600 pages the volume contains about 1300 pages. When bound, a volume of this size would be unmanageably large and it has therefore been decided to divide it into two Sections of approximately equal size and to provide a title page for each.

The concluding Parts (Parts 41 and 42) have been kindly prepared on the invitation of the Trust by Mr. Francis Hemming as having been Secretary to the Commission and Editor of the *Bulletin* during the period in which the earlier Parts of this volume were published.

(*Signed*) **HURCOMB**

Office of the International Trust
for Zoological Nomenclature,
41, Queen's Gate,
LONDON, S.W.7.

4th September 1958.

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

PURCHASED
14 JUL 1958

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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REPORT ON THE WORK CARRIED OUT BY THE INTERNATIONAL TRUST FOR ZOOLOGICAL NOMENCLATURE IN THE FIVE-YEAR PERIOD 1953–1957

PURCHASED
14 JUL 1958

Report prepared by the International Trust for Zoological Nomenclature for consideration by the International Union for Biological Sciences at its General Assembly to be held in London in July 1958

In the present Report the International Trust for Zoological Nomenclature has pleasure in placing before the International Union for Biological Sciences a Report on the work carried out by it on behalf of the International Commission on Zoological Nomenclature in the five-year period 1953–1957. As will be seen, the period was one of great achievement, both scientific and financial.

2. At the time of the submission of its last general Report in 1953 the International Trust had just carried through with great success a Colloquium on Zoological Nomenclature which with the assistance of grants from the International Union for Biological Sciences and from U.N.E.S.C.O. it had organised in connection with the meeting at Copenhagen of the Fourteenth International Congress of Zoology. The purpose of that Colloquium was to provide an internationally agreed basis for the completion of the revision of the *Règles Internationales de la Nomenclature Zoologique* (International Code of Zoological Nomenclature). In this the Colloquium proved highly successful, for the whole of its recommendations were ultimately approved by the Congress. This satisfactory result was largely due to the careful preparation of the ground by the Trust and to the allocation for this purpose of three volumes (Volumes 7, 8 and 10) of the *Bulletin of Zoological Nomenclature*.

3. The time and effort devoted to the preparations for the Copenhagen Colloquium were well justified by the results secured, but inevitably involved some sacrifice in the normal work of the Commission and the Trust. The first task after the close of the Colloquium was the preparation and piloting through the press of the volume containing the Official Record of the decisions taken by the Copenhagen Congress on the basis of the recommendations of the Colloquium. This volume which was published under the title *Copenhagen Decisions on Zoological Nomenclature* was issued on 31st December 1953.

4. The fact that it was possible during the summer of 1953 to carry through the preparations for the Copenhagen Colloquium on the scale and with the speed achieved was due mainly to the fact that in April of that year Mr. Francis Hemming, Secretary to the Commission, had retired from the British Civil Service and had started to work on a whole-time (though unpaid) basis instead of only in his spare time which alone previously he had been able to give to this work. It was evident that once the work arising out of the Colloquium was completed, the Office of the Commission would on the above account be in possession of resources far greater than had ever previously been at its disposal. Careful consideration was given by the Trust in the autumn of 1953 to the administrative problems involved for the purpose of devising the arrangements best calculated to take full advantage of the opportunity so offered. Clearly, the Secretary by working on a full-time (unpaid) basis would be able to increase the level of output far above that ever previously achieved. Thus, by the maintenance of the Trust's established price-policy it would be reasonable to look for a small profit on the sale of each of the increased number of units which it would be possible for the Trust to publish. The Trust took the view that the fullest possible advantage should be secured from having a whole-time Secretary and formed the conclusion therefore that the proper course would be to appoint a salaried Administrative Officer to relieve Mr. Hemming of as much as possible of the routine or semi-routine duties which he would otherwise have had to perform, thereby freeing him to concentrate as far as possible upon the purely scientific side of the work. It was hoped that by this means the number of publications issued annually would be increased to an extent which would not only cover the salary of the proposed Administrative Officer and the consequent increase in general office expenses but would in addition, through the increase in the number of units published annually, actually lead to an increased net profit. As a contribution towards the success of this plan, Mr. Hemming offered to provide office accommodation for the reinforced staff of the Commission in his own private house without any charge in respect of rent. The plan described above obviously involved certain risks but on the other hand it alone offered a possibility of enabling the Office of the Commission to make substantial inroads into the arrears of applications which had inevitably accumulated when the Secretary had been able to work only on a spare-time basis in the evenings and at week-ends.

5. The new plan was brought into operation in November 1953 and the first of the new series of documents was published at the end of January 1954. It very quickly became apparent that the rate of production and publication could be still further increased by the appointment of additional staff accompanied by a further devolution of duties by the Secretary. A number of such appointments were made in the course of 1954 and these still further swelled the volume of current output. In the period preceding the Copenhagen Congress the major part of the work of the Office of the Commission—other than that concerned with the preparations for the Copenhagen Colloquium—had been concerned with the production of units of the Trust's serial, the *Bulletin of Zoological*

Nomenclature, containing applications on individual problems submitted to the Commission for decision. A large number of decisions had been taken by the Commission on such cases largely by postal vote but the decisions so taken had not been promulgated as *Opinions*. The Trust accordingly decided that its new programme should consist of two branches, namely the preparation and publication of *Opinions* on individual cases on which decisions had already been taken, and, second, the preparation of further cases for consideration by the Commission through publication in the *Bulletin of Zoological Nomenclature* and later through the issue of Voting Papers on the questions raised in those cases. The success of the new policy described above was both immediate and complete. The number of publications issued rose rapidly, a small profit was made on each item sold and the income so obtained proved to be more than sufficient to cover the cost of employing salaried staff to assist the unpaid whole-time Secretary.

6. The first units published—in 1954—under the new Publications Programme were units of the *Opinions and Declarations* Series. In the above year no less than 139 Parts (including three index Parts) of this Series were published at a printing cost of £5,110. Income from sales of Parts of this Series (including the sale of back-parts) amounted to £5,704, thus providing a net profit of about £600 during the year. Similar results were obtained in each of the three following years. Throughout that period the conditions obtaining were strictly comparable and it is therefore very gratifying to note that, while expenditure on printing was at a somewhat lower level, income from sales showed a steady increase, as the result partly of increases in the number of subscriptions and partly of sales of complete back-sets, the cost of which increased rapidly during the period under review. As the result of these favourable factors sales of units of the "*Opinions and Declarations*" Series exceeded the cost of printing by about £2,000 in 1955, by £3,000 in 1956 and by over £3,500 in 1957. The total output published during the five-year period 1953–1957 amounted to 432 Parts (including 15 index Parts) and contained over 8,000 pages.

7. The publication of units of the *Bulletin of Zoological Nomenclature* under the new programme during the period 1954–1957 produced results very similar to those described for the *Opinions and Declarations* Series. In 1954, for example, printing costs amounted to £1,200, while sales (current sales and sales of back Parts) amounted to £2,200, thus providing a contribution of about £1,000 towards the general overhead costs of the work of the Commission. In the following year (1955) printing costs amounted to £1,030 and income from sales £1,980, while for 1956 the figures were £1,950 and £3,700. For 1957 about one half of the expenditure incurred was on "normal" *Bulletin*, the remainder being on Parts of the London Congress Agenda Volumes (Volumes 14 and 15). The total expenditure so incurred amounted to £3,140 and income to £6,040. Thus, the total expenditure incurred on printing the *Bulletin* in the period 1954–1957 amounted to approximately £7,300 and income from sales (including back sales) to £13,900, the net contribution to the general income of the Commission amounting on the average to £1,650 per annum during the period under review. The total number of Parts (including six index Parts) published during the four-year period amounted to 85 (3,250 pages).

8. The adoption in 1953 of the plan described above for launching a greatly enlarged Publications Programme was, as has been seen, fully justified in the event but at the time when it was initiated, it necessarily involved considerable risks, as at that time it was impossible to be certain that the increased output to be looked for would provide a volume of sales sufficient to cover the cost to the Trust of the salaried staff required for their production. Up to that time (i.e. up to the end of the year 1953) administrative expenses chargeable to the Income and Expenditure Account had amounted to about £500 per annum. In 1954 the new Publications Programme led to an increase of about £800 in this item which amounted in that year to about £1,300 ; in 1955 this figure was increased to about £1,500. Further developments in the work of the Office raised expenditure under this head to about £2,500 in each of the years 1956 and 1957. As in previous periods the expenditure shown under this head throughout the period under review was substantially less than that which would have been involved if the Office of the Trust had been organised on ordinary business lines. For in these years the Offices of the Trust and the Commission were accommodated rent-free in Mr. Hemming's private residence, while Mr. Hemming discharged the duties of Secretary on a whole-time basis without remuneration.

9. During the period under review the Trust—and through the Trust the Commission also—was extremely fortunate in the matter of gifts received. First, it is necessary to record that in 1953 at the time of the launching of the new Publications Programme Mr. Hemming presented a gift of £1,760, the largest single benefaction ever received by the Trust. During the same period also the International Union for Biological Sciences continued to mark its interest and that of U.N.E.S.C.O. in the work of the Trust and the Commission by making financial contributions within the limitations imposed by its own budgetary position. During the four-year period 1954–1957 the amount so presented to the Trust amounted to £857. In addition, gifts of small amounts from various institutions and individuals amounted in the aggregate to the sum of £106 during the four years in question. To all those from whom gifts were received during the period under review the grateful thanks of the Trust are offered.

10. Reference must be made at this point to the exceptional arrangements made by the Trust in 1953 to secure the widest possible publicity in the scientific world for the decisions on nomenclature taken by the Copenhagen Congress in 1953. Two considerations were judged to be of special importance, namely speed of publication and a low price for the volume to be published. The work itself was issued under the title *Copenhagen Decisions on Zoological Nomenclature* at the end of December 1953—that is, little more than four months after the close of the Congress. In order to ensure that this vital work should be obtainable at a price so low as to be within the reach of any institution or individual the Trust decided to grant a substantial subsidy to its production and thus to render it possible to place it on sale at the nominal price of five shillings a copy. The total number of copies sold of this first impression amounted to about 900. At the time when the stock was exhausted there remained only a small unsatisfied demand. In view, however, of the importance of this work it was decided by the Trust to issue a Second Impression. By the end of 1957 about 100 copies of this new impression had been sold. The total sales achieved thus amounted only to about 1,000 copies, in spite of the fact that the work itself was one of much more general interest than are most works dealing with zoological nomenclature and that the price charged for the principal edition was fixed at an artificially low level. The experience gained from the publication of this book is, in the view of the Trust, very instructive, for it shows that the potential field for the sale of books and serials dealing with zoological nomenclature is extremely limited, not because of lack of interest in the subject but because under modern conditions the private purchaser has almost disappeared, his place having been taken by institutional libraries serving large numbers of individuals.

11. During the period under review great attention has been devoted by the Trust to two special projects of great importance, each of which has now been brought virtually to its conclusion. The first of these was concerned with the publication in book-form of the *Official Lists* of valid names and of the *Official Indexes* of rejected and invalid names ; the second was concerned with the preparations for the publication of the Draft of the text of the *Règles Internationales de la Nomenclature Zoologique* (International Code of Zoological Nomenclature) as revised by the International Congresses of Zoology held in Paris in 1948 and at Copenhagen in 1953. Brief particulars of the action taken by the Trust under each of these heads are given below.

12. The plan for the establishment of an *Official List of Generic Names in Zoology* was approved by the International Congress of Zoology held at Monaco in 1913, the purpose of the *List* being to promote stability in the names for important genera. Owing first to the First World War and later to the spirit of defeatism in matters of nomenclature which marked the greater part of the inter-war period the *List* grew very slowly, the number of names inscribed on it by the end of 1936 amounting only to 563. By this time it had become apparent that the value of the *Official List* was severely prejudiced by the fact that the decisions placing names on it were scattered over a large number of separate issues of serial publications and that what was required was a consolidated edition of the *Official List* published in book-form. It was then found that the earlier entries on the *Official List* had been made in so condensed (and often incomplete) a form that a considerable amount of further work would be required before publication in book-form would be practicable. For this work it was decided to set up a special " Official List Section " in the Office of the Trust and to engage a qualified zoologist to take charge of this Section on a whole-time basis. These arrangements were brought into operation in September 1954. Concurrently with the examination of the entries on the foregoing *Official List* made in the period up to the end of 1936 work was started also on the preparations for the publication in book-form of the *Official Lists* for valid specific names established by the Paris Congress in 1948 and for valid family-group and Order/Class names and for valid zoological works established by the Copenhagen Congress in 1953. At the same time work was started on the corresponding *Official Indexes* of rejected and invalid names and works. Almost all the problems outstanding in connection with generic names placed on the *Official List* in the period up to the end of 1936 had been cleared up by the summer of 1957. It was thereupon decided that arrangements should be put in hand forthwith for the publication in book-form of the First Instalments of all the *Official Lists* and *Official Indexes*, even though in the case of generic names this might involve a few temporary omissions from the *List* so published. These volumes have now in some cases been published and it is hoped that the publication of the remainder will be completed in time for the Fifteenth International Congress of Zoology when it meets in London next month (July). The total expenditure incurred on this important project during the four-year period 1954–1957 amounted to the modest figure of £1,950. The prices charged for the volumes now being published are very moderate but from the advance orders already received it seems likely that the actual cost of printing—though not the cost of preparation—will be recovered in 1959.

13. The second of the two important projects which have engaged the attention of the Trust during the period under review has been the arrangements to be made for securing the approval of the Fifteenth International Congress of Zoology, London, 1958, for a text of the *Règles Internationales* (International Code) based upon the decisions taken by the Paris and Copenhagen Congresses in 1948 and 1953 respectively. In this connection it will be recalled that, while the actual preparation of the first Draft of the revised text of the *Règles* was confided to Professor J. Chester Bradley (*Ithaca, N.Y.*), the publication of that Draft and all the subsequent arrangements necessary for its due consideration were entrusted to the International Trust. The compilation of the proposed document would, it was apparent, be a highly complex task, having regard to the very large number of separate points to be covered. Accordingly, in agreement with Professor Bradley, the Trust established a special " *Règles* Section " to advise on all problems arising in connection with the Draft of the *Règles*. Somewhat later, arrangements were put in hand by the Trust for summoning a Colloquium on Zoological Nomenclature for the purpose of considering the Draft of the *Règles* and of proposals for the further amendment of the *Règles* or for the correction of the Draft prepared by Professor Bradley received either from the members of the Colloquium or from others. At the same time the Trust earmarked two volumes of the *Bulletin of Zoological Nomenclature* to serve as Agenda Volumes for the London Congress. The first of these volumes (Volume 14) was reserved for Professor Bradley's Draft of the English Text and for the Draft of the corresponding French text ; the second (Volume 15) was made available for the publication of comments of any kind on Professor Bradley's Draft. The total expenditure incurred in connection with the above matter during the three-year period 1955–1957 (including the expenditure incurred on the publication in 1957 of the first instalments of each of the above volumes) amounted to £2,570.

14. Having now examined both the Trust's sources of income during the four-year period 1954–1957 and the expenditure incurred during those years both on the normal services of the Trust and on the two special projects (*Official Lists* ; *Règles*) on which expenditure was incurred during that period, it is at length possible to determine the overall financial position of the Trust, as measured in terms of the difference annually between total income and total expenditure. In 1954, the first of the four years under review, the Trust had to bear the whole of the additional expenditure on salaries involved in its new Publications Plan, while it could not be hoped that in that year it would be able to build up the support from subscribers needed to make that programme a financial success. In these circumstances it was very gratifying that in that year a net excess of income over expenditure amounting to £660 should have been obtained. In each of the succeeding years the favourable results so obtained were not only maintained but greatly enhanced. In 1955 income exceeded expenditure by £1,780 ; in 1956 by £2,631 ; in 1957 by £5,802. These figures do not take account of the transfers made to the subsidiary accounts from which the "*Official Lists*" and "*Règles* (Preparation)" projects were financed. The buoyant state of the finances of the Trust may best be judged by reference to its Revenue Reserves. These reserves at the end of 1957 amounted to £9,669, an increase of £6,300 over the level at which they had stood at the end of 1953.

15. In the conduct of operations such as thus for which the Trust is responsible considerable liquid assets are required both as an insurance against adverse contingencies and in order also to provide the substantial amount of working capital needed to finance the printing of publications before the actual receipt of funds arising from the sale of units of the immediately preceding instalments. Subject to the fulfilment of these conditions, the accumulation of reserves has never been more than a secondary purpose of the Trust's financial policy, its prime object having been to build up its income to a level which would make it possible to support the cost involved whenever, on a change taking place in the Secretaryship of the Commission, the subsidies represented by the whole-time unpaid service provided by Mr. Hemming as Secretary and by the provision of rent-free office accommodation in Mr. Hemming's private house were no longer available. For it was apparent that, whenever such a change were to take place, it would be necessary to engage at the full market rate a senior zoologist to take charge of the Office of the Commission. Such an official, being in a receipt of a salary from the Trust, could not be a member of the Commission and could not therefore be appointed to the Office of Secretary as hitherto constituted. On the other hand, once a senior zoologist had been appointed to take charge of the Office on a whole-time salaried basis, there would clearly be no room for a spare-time Honorary Secretary drawn from the membership of the Commission, for there would be no effective functions for such an honorary officer to perform and the existence of such a post could at the best lead only to duplication of effort and delay—through the need for otherwise unnecessary correspondence between the salaried head of the Office and the Honorary Secretary who almost certainly would be a person resident in some other country. The Trust has accordingly always considered that, when Mr. Hemming gave up his present honorary post, it would be necessary not only that his whole-time salaried successor should be a specialist whose personal achievements should command respect from the Commission's correspondents but also that he should be a man of considerable seniority and thus qualified to take full charge of the Office of the Commission, to which body he should be directly responsible. As will be appreciated, the nature of the salaried post so to be created is a matter of direct concern to the Trust in view of its responsibilities for providing the financial and technical resources required for the continuance of the work of the Commission.

16. It is a matter of great satisfaction to the Trust that when suddenly some months ago Mr. Hemming found it necessary on the advice of his doctors to ask to be relieved of his duties as Secretary to the Commission, the financial position which had been built up in recent years made it possible for the Trust at once to create the required post at the current market rate of salary—a minimum of £2,500 per annum—and to rent at a moderate figure (£225 per annum) office accommodation for the Commission which, though very modest and small, is nevertheless just sufficient to meet essential requirements at the present time. Finally, with the active assistance of Sir Gavin de Beer, President of the Fifteenth International Congress of Zoology, and with the friendly co-operation of the British Government Departments concerned, the Trust was able to appoint Mr. R. V. Melville, a senior palaeontologist on the staff of the Geological Survey of the United Kingdom, to the new post of Assistant Secretary to the Commission and Director of its Office, for a period initially of one year. At the same time Mr. Melville was appointed to be an Assistant Manager to the Trust, the post of Managing Director being retained for the time being by Mr. Hemming. The Trust is confident that the arrangements described above are the best that in the circumstances could have been devised and will ensure that the work of the Office is continued without interruption or disturbance. Mr. Melville will, however, need to be given all possible support by interested zoologists and the Trust does not doubt that this support will be forthcoming.

OFFICES OF THE INTERNATIONAL TRUST FOR
ZOOLOGICAL NOMENCLATURE,
41 QUEEN'S GATE,
LONDON, S.W.7.

25th June 1958

PURCHASED
1958

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The Official Organ of

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Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

A. The Officers of the Commission

Honorary Life President: Dr. Karl JORDAN (*British Museum (Natural History), Zoological Museum, Tring, Herts, England*)

President: Professor James Chester BRADLEY (*Cornell University, Ithaca, N.Y., U.S.A.*) (12th August 1953)

Vice-President: Senhor Dr. Afranio do AMARAL (*Sao Paulo, Brazil*) (12th August 1953)

Secretary: Mr. Francis HEMMING (*London, England*) (27th July 1948)

B. The Members of the Commission

(Arranged in order of precedence by reference to date of election or of most recent re-election, as prescribed by the International Congress of Zoology)

Professor H. BOSCHMA (*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*) (1st January 1947)

Senor Dr. Angel CABRERA (*La Plata, Argentina*) (27th July 1948)

Mr. Francis HEMMING (*London, England*) (27th July 1948) (*Secretary*)

Dr. Henning LEMCHE (*Universitetets Zoologiske Museum, Copenhagen, Denmark*) (27th July 1948)

Professor Pierre BONNET (*Université de Toulouse, France*) (9th June 1950)

Mr. Norman Denbigh RILEY (*British Museum (Natural History), London*) (9th June 1950)

Professor Dr. Tadeusz JACZEWSKI (*Institute of Zoology, Polish Academy of Sciences, Warsaw, Poland*) (15th June 1950)

Professor Dr. Robert MERTENS (*Natur Museum u. Forschungs-Institut Senckenberg, Frankfurt a. M., Germany*) (5th July 1950)

Professor Dr. Erich Martin HERING (*Zoologisches Museum der Humboldt-Universität zu Berlin, Germany*) (5th July 1950)

Senhor Dr. Afranio do AMARAL (*S. Paulo, Brazil*) (12th August 1953) (*Vice-President*)

Professor J. R. DYMOND (*University of Toronto, Toronto, Canada*) (12th August 1953)

Professor J. Chester BRADLEY (*Cornell University, Ithaca, N.Y., U.S.A.*) (12th August 1953) (*President*)

Professor Harold E. VOKES (*University of Tulane, Department of Geology, New Orleans, Louisiana, U.S.A.*) (12th August 1953)

Professor Béla HANKÓ (*Mezőgazdasági Muzeum, Budapest, Hungary*) (12th August 1953)

Dr. Norman R. STOLL (*Rockefeller Institute for Medical Research, New York, N.Y., U.S.A.*) (12th August 1953)

Mr. P. C. SYLVESTER-BRADLEY (*Sheffield University, Sheffield, England*) (12th August 1953)

Dr. L. B. HOLTHUIS (*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*) (12th August 1953)

Dr. K. H. L. KEY (*Commonwealth Scientific and Industrial Research Organisation, Canberra, A.C.T., Australia*) (15th October 1954)

Dr. Alden H. MILLER (*Museum of Vertebrate Zoology, University of California, Berkeley, California, U.S.A.*) (29th October 1954)

Doc. Dr. Ferdinand PRANTL (*Národni Museum v Praze, Prague, Czechoslovakia*) (30th October 1954)

Professor Dr. William KÜHNELT (*Zoologisches Institut der Universität, Vienna, Austria*) (6th November 1954)

Professor F. S. BODENHEIMER (*The Hebrew University, Jerusalem, Israel*) (11th November 1954)

Professor Ernst MAYR (*Museum of Comparative Zoology at Harvard College, Cambridge, Massachusetts, U.S.A.*) (4th December 1954)

Professor Enrico TORTONESE (*Museo di Storia Naturale "G. Doria", Genova, Italy*) (16th December 1954)

Dr. Per BRINCK (*Lunds Universitets, Zoologiska Institution, Lund, Sweden*) (19th May 1958)

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PAPERS CIRCULATED TO THE COLLOQUIUM FOR INFORMATION

PURCHASED
21 JUL 1958

DOCUMENT A*

REPORT DATED 20TH JUNE 1958 SUBMITTED BY THE INTERIM COMMITTEE ON ZOOLOGICAL NOMENCLATURE IN RESPECT OF THE PERIOD 1953–1958 ADDRESSED TO PROFESSOR J. CHESTER BRADLEY, PRESIDENT OF THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

[*Communicated to the Colloquium for information by direction of the President of the Congress*]

Leiden, 20*th June* 1958

Dear Mr. President,

We have the honour to submit with this letter the Report of the Committee appointed at the Final Concilium Plenum of XIVth International Congress of Zoology. This Committee was charged with the duty of " implementing " the administrative and financial plan drawn up at Copenhagen by a Committee presided over by Professor Spärck, President of the XIVth Congress for the support of the International Commission on Zoological Nomenclature and the reorganisation of that Commission's Secretariat.

The Committee was instructed to put the Copenhagen Plan into effect immediately upon the resignation of Mr. Francis Hemming as Honorary Secretary of the Commission. As explained in the Report, Mr. Hemming expressed his intention, at Copenhagen, of retiring from the office of Honorary Secretary some time during the inter-Congress period. In fact, his resignation has been delayed until the close of the London Congress. Under these circumstances, the Committee are also submitting, for the consideration of the

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*For Document B see page xxxiii.

International Commission on Zoological Nomenclature, and of the XVth Congress of Zoology, their Recommendations for the implementation of the Copenhagen Plan. These Recommendations would already have been put into force had the delay in Mr. Hemming's resignation not taken place.

Respectfully yours :

H. Boschma, *Chairman* ;
L. B. Holthuis,
P. C. Sylvester-Bradley,
R. L. Usinger.

Interim Committee.

To :

Professor J. Chester Bradley, President, International Commission on Zoological Nomenclature.

Copies transmitted, on the instructions of Professor J. Chester Bradley, to :—
Sir Gavin de Beer, F.R.S., President, XVth International Congress of Zoology.

ENCLOSURE TO LETTER FROM THE INTERIM COMMITTEE DATED 20TH JUNE 1958

International Congress of Zoology
Interim Committee on Zoological Nomenclature
Report : 1953–1958

1. Terms of Reference

The Interim Committee was established at the Final Concilium Plenum of the XIVth International Congress of Zoology at Copenhagen in August, 1953, on the proposal of the Permanent Committee of the Congress and of the International Commission on Zoological Nomenclature. It was charged with the implementation of a Plan, adopted by the Congress in Plenary Session, for the transfer of the Secretariat of the International Commission on Zoological Nomenclature, and the reorganisation of the arrangements for the financial support of the Commission. This Plan has been formulated in response to the intimation of Mr. Francis Hemming, Secretary to the Commission, of his intention of resigning during the forthcoming inter-congress period. The Plan provided for :—

1. the organisation of a new financial structure on the basis of an International Association formed for the study of zoological nomenclature in general and for support of the International Commission in particular ;

2. the re-allocation of the arduous duties which were then carried on at great personal sacrifice by the Honorary Secretary. Some of these duties, including financial matters, were to be handled by the Association through its officers and members. General matters relating to publications were to be placed in the hands of an Honorary Editor.

2. Composition of the Interim Committee

The following four gentlemen were elected to the Interim Committee :—

Dr. H. Boschma, Chairman ;

Mr. P. C. Sylvester-Bradley, Secretary, and Interim-Secretary-Designate ;

Dr. L. B. Holthuis, Interim-Editor-Designate ;

Dr. Robert L. Usinger.

3. Text of the Reports adopted at Copenhagen

The terms of reference and constitution of the Interim Committee, as summarised above, were embodied in two Reports which were presented to the Final Concilium Plenum of the Congress, and were there approved and adopted. The circumstances attending the preparation of these Reports and their subsequent history is briefly indicated in footnote No. 94, on p. 94, of the *Copenhagen Decisions* (1953). The Plan embodied in these Reports (for convenience, referred to as the " Copenhagen Plan ") was drafted by a Committee of which Professor Spärck, President of the Congress, acted as Rapporteur, and by a meeting of the International Commission convened to consider Professor Spärck's Report. The footnote concludes with the statement : " The two Reports will be published shortly in the *Bulletin of Zoological Nomenclature* ". Up to the present they have not been so published, however, and their text is therefore appended as Annexes 1 and 2 to the present Report.

4. Financial Support of the Interim Committee

No arrangements were made at Copenhagen for the financial support of the Interim Commission. It is therefore a pleasure to record that the Entomological Society of America and the Society of Systematic Zoology each voted the Committee fifty dollars from their funds. These funds have enabled the Committee to meet the costs of stationery, postage and administration, and an account of the expenditure occurred is attached to this Report as Annexe 3.

5. Arrangements for the transfer of the Secretariat to Sheffield

The date fixed at Copenhagen for the introduction of the Plan there formulated was to be the date upon which the new Rules, as revised at the Paris and Copenhagen Congresses, were published by the International Trust. In order to render possible this arrangement, Mr. Hemming formally notified the Commission that the date of his resignation of the Office of Secretary should be timed to coincide with this event. At that time it was supposed that this would take place some time in 1955, and Mr. P. C. Sylvester-Bradley was instructed (in terms adopted by the Congress in plenary session) to take up office at that date as Interim Secretary and Dr. L. B. Holthuis was similarly instructed to take up duties as Interim Editor. The Interim Committee therefore put in hand arrangements for the transfer of the Secretariat to Sheffield, England, where Mr. Sylvester-Bradley is employed as a member of the University. The authorities of the Sheffield City Museum offered to put at the disposal of the Commission rent-free accommodation for the offices of the Secretariat.

6. Mr. Hemming's decision to postpone the date of his retirement

Mr. Hemming, on 15th May 1954, wrote to the Secretary of the Committee intimating that various changes in the situation since August 1953, had forced him to the conclusion that it was no longer desirable to promulgate the new Rules in the form that had been proposed at Copenhagen, but rather that the text should be published in Draft form only, for consideration by a Colloquium which he suggested should be summoned to meet in London during 1958, just before the Congress. As a result of this conclusion, Mr. Hemming also indicated that he proposed to delay the date of his retirement until some time after the London Congress.

7. Arrangements for the Presentation of the Interim Committee's Report and Recommendations

Mr. Hemming also included in his letter of the 15th May, 1954, the following statement :—

> " The fact that the Revised Code will not be ready as soon as was hoped for at Copenhagen means . . . that the Copenhagen Committee will have more time to examine the administrative and financial issues with which they are concerned than was originally expected ".

After due consideration the Committee, on 12th October 1954 in a reply to Mr. Hemming, agreed that the new timetable would have certain advantages, and indicated that they proposed to lay their plans before the London Congress, so that, if approved, the new *International Association for Zoological Nomenclature* could be inaugurated at the final Plenary Session.

8. Mr. Sylvester-Bradley's visit to California

At the request and by the generosity of various departments in the University of California, Stanford University, and the California Academy of Sciences, Mr. Sylvester-Bradley spent a week in California during May 1956, discussing the plans of the Interim Committee at a series of meetings. The audiences addressed expressed enthusiastic and unanimous support at the prospect of the foundation of a new International Association for Zoological Nomenclature, with its promise of wider and less expensive circulation of publications relating to Zoological nomenclature.

9. Public Notification of Mr. Hemming's Retirement

On 29th April 1958, Mr. Hemming gave public notification of his impending retirement, and announced that, on the grounds of ill-health, he would not, on the completion of his present term of service in that office, allow himself to be nominated for a further term of service.

The Interim Committee took the view that the plan they had prepared for the consideration of the coming Congress gained, as a result of Mr. Hemming's announcement, a further degree of urgency, and resolved as a consequence to call a special session of the Committee in Leiden on 19th and 20th June 1958, at which the Recommendations they had earlier drawn up could be revised, where necessary, to cater for recent developments. At the same time the Committee informed Professor J. Chester Bradley, President of the International Commission of their plans and present arrangements, and Professor Bradley graciously consented to act as a consultant to the Committee, and offered any help that he could give.

10. The Committee's Recommendations

The Committee adopted and approved the Recommendations which they present currently with this Report and which, in accord with the instructions given them in Copenhagen, outline a Plan for the inauguration of an *International Association for Zoological Nomenclature*, and set out a detailed programme for the re-organisation and financial support of the Secretariat of the International Commission which, they suggest, should come into operation immediately Mr. Hemming's retirement becomes effective—i.e. from the close of the London Congress on Wednesday, 23rd July 1958.

Appendix to the enclosure to the letter of the Interim Committee dated 20th June 1958

RECOMMENDATIONS
of the
INTERIM COMMITTEE
for the implementation of the
COPENHAGEN PLAN
for the financial support and administrative
organisation of the
INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Introduction

1. The authority of the Interim Committee and its terms of reference are as set out in the Report of the Committee submitted concurrently with the present Recommendations.

2. The Recommendations that follow are divided into five sections :—

(a) General Financial Policy ;
(b) The International Association for Zoological Nomenclature ;
(c) Publication ;
(d) Reorganisation of the Secretariat ;
(e) Budget.

Section A. GENERAL FINANCIAL POLICY

3. During all but the last few weeks of the period from the close of the Copenhagen Congress until the opening of the London Congress, the Secretariat of the Commission has occupied rent-free accommodation by courtesy of the Honorary Secretary, Mr. Francis Hemming. On 10th June 1958, the Secretariat moved to rented accommodation at 119, Parkway, London, N.W.1. The amount of rent on this accommodation has not yet been divulged.

4. During the whole of the same period, Mr. Hemming has acted as Secretary to the Commission in a purely honorary capacity. A gradually increasing staff of clerical and technical assistants has been employed in the

Secretariat, however, and in the last Report issued (for 1956) the total salary expended on such assistance was given as £1,262. This compares with £117 for 1954. Since 1st May 1958 Mr. R. V. Melville has been appointed as a full-time salaried Assistant Secretary.

5. The amount debited during this period to office expenses has risen from £498 (1954) to £1,282 (1956).

6. The cost of printing the *Bulletin* and the *Opinions* has been as follows :—

1954	£6,321
1955	£4,694
1956	£6,184

7. The total annual cost of running the Secretariat in its present form including printing costs amounted to £8,670 in 1956. The developments which have taken place since 1956 suggest that at least an additional £3,000 will be needed. £12,000 must be regarded as a conservative estimate of the annual income needed to support the Secretariat and its publications as at present organised.

8. The financial support of the Secretariat has been derived in a small part from donations. The much larger remaining part (£10,939 in 1956) has been derived from the sale of publications.

9. The greatly increasing costs of running the Secretariat have been more than met by increased profits from the sale of publications. This increased profit has been achieved not by any great increase in circulation, but rather by an increased output, with a consequent increase in the cost to individual subscribers. Every page added to the *Bulletin* results in more profit. The cost to those who have subscribed to both the *Bulletin* and the *Opinions* during the years since the Copenhagen Congress is as set out below :—

	Bulletin	*Opinions*	*Total*
1954	£10	£50	£60
1955	£8	£46	£54
1956	£19	£62	£81
1957	£33	£74	£107

The amount of the annual subscriptions places the publications of the Commission beyond the reach of any individual zoologist, and, indeed, beyond the reach of a great many institutions. Only those zoologists who have access to the libraries of the greater or more wealthy institutions can have recourse to the publications and it can be calculated that the total number of institutions who subscribed to both series of publications must have been fewer than 135 in 1956. The Committee regard such a restricted circulation with the greatest misgiving and fear that the further reduction in circulation, of which there are already signs, can only result in the complete negation of the Commission's authority.

10. At Copenhagen, President Spärck's Committee were conscious of the strong feelings that had been expressed in opposition to a publication policy such as that outlined above. It was indeed in consideration of the disadvantages attendant on a financial policy based on high-priced publications that the Copenhagen Plan was formulated. The object is as set out in Decision 184 of the *Copenhagen Decisions* (p. 95) :

> " It is therefore of the highest importance that a new financial basis should be devised as soon as possible for supporting the work of the Commission which will be sufficiently strong and assured to make it possible to place the *Bulletin* and the *Opinions* of the Commission on sale at prices which will put these publications within the reach of all zoologists ".

11. The financial situation has greatly worsened (from the point of view of the zoological public) since Copenhagen, and the Interim Committee have been aghast at the ever-increasing burden that has been laid on subscribers to the publications. The Recommendations here put forward envisage a drastic economy of expenditure, and the creation of an entirely new source of income, designed to render the expenses of the Secretariat gradually less dependent on profits made from the sale of publications.

12. The Committee recognise the undesirability of terminating the present source of income in favour of an untried and still hypothetical alternative. They recommend that the change should be controlled in such a way that the profit on sale of publications should be reduced gradually and only as the introduction of economies and the new source of income become effective.

13. The economies that the Committee have in mind include :—

(a) a drastic reduction in the number of salaried staff employed by the Secretariat ; the Committee believe the employment of a salaried Secretary to be both unjustified and undesirable ; the details of the Committee's recommendations are included in Section D below.

(b) the abandonment of rented accommodation ; many institutions are proud of their record in support of such scientific activities as those of the Secretariat and the Committee has secured the promise of accommodation which would be provided free of rent if the

Committee's Recommendations are adopted by Congress; details are again given in Section D below.

(c) a drastic reduction in the length and number of the publications, and their re-organisation to avoid the present duplication between the *Bulletin* and the *Opinions*; details are in Section C.

14. The new source of income envisaged by the Committee is that which would be derived from the various classes of subscription to an International Association for Zoological Nomenclature. It is recommended that this Association be inaugurated by the present Congress. Details are set out in Section B.

Section B. THE INTERNATIONAL ASSOCIATION FOR ZOOLOGICAL NOMENCLATURE

15. The most important task entrusted to the Interim Committee was the organisation of the International Association for Zoological Nomenclature.

16. This Association is intended to attract a large individual membership of zoological taxonomists. Its first purpose will be to provide a regular income for the support of the Secretariat of the Commission. Its second and no less important purpose will be to promote a wide distribution of the publications of the Commission, and thus aid the important new procedures laid down at Copenhagen whereby regular reference to the *Bulletin of Zoological Nomenclature* will be an essential part of the work of all zoologists aiming towards a stable and universally accepted nomenclature.

17. The Interim Committee has realised that it is important to the success of the Copenhagen Plan that the subscription rates to the new Association should be low enough to attract a very wide support from individuals. The annual subscription rate recommended is that of £1 1s. If the Association is, with this low subscription rate, to fulfil its financial obligations, it will be necessary to ensure that in fact such a wide support is achieved. It is with this in mind that the Committee recommends that the Association should issue free to every member on joining a copy of the new Rules.

18. It is proposed that the Association should be composed of the following classes of members:

(a) *Individual Members*:

(i) *Ordinary*: Entrance fee 7s. ($1.00)
Annual subscription £1 1s. ($3.00)

(ii) *Sustaining*: Entrance fee 7s. ($1.00)
Annual subscription £5 5s. ($15.00)

(iii) *Patrons*: Single donation of £100 ($300) or more.

(b) *Institutional Members* :

- (i) *Ordinary* : Annual subscription £1 15s. ($5.00)
- (ii) *Sustaining* : Annual subscription £15 ($45.00)
- (iii) *Patrons* : Single donation of £100 ($300) or more.

All members would receive the following privileges :—

- (i) a free copy of the RULES on joining ;
- (ii) the JOURNAL free of charge ;
- (iii) all other publications at a reduced price ;
- (iv) the right to vote at General Meetings and at Colloquia arranged from time to time to discuss nomenclatorial matters.

Institutional members are to have the right to send representatives to General Meetings and Colloquia. Individual patrons to receive the privileges of ordinary membership for life. Institutional patrons to receive the privileges of Institutional members for ten years.

19. It is recommended that the Honorary Officers of the Association should be three : the President (who will be, *ex officio*, the President of the Commission) ; Secretary ; and General Treasurer. There should also be local Treasurers established in all countries where there is any considerable local body of support.

20. The Council of the Association should consist of :—

- (i) the honorary officers of the Association (President, General Treasurer, Secretary) ;
- (ii) the honorary officers of the Commission (Vice-president, Joint Secretaries) ;
- (iii) five other members to be elected at a general meeting.

21. In certain countries it is hoped that it may be possible to appoint Sponsoring Societies, who will undertake through their own officers the organisation of the affairs of the Association within the country concerned. In such cases the Sponsoring Society would nominate from amongst its members a Local Treasurer of the Association. It is recommended that the Society of Systematic Zoology (which has over a thousand members) be invited to become a Sponsoring Society for the U.S.A.

22. The Association should be responsible for the organisation from time to time of both national and international meetings for the discussion of matters of nomenclatorial interest.

23. It is recommended that the Congress consider inaugurating the new International Association at the Final Concilium Plenum, planned to take place on the morning of Wednesday, July 23rd. It is suggested that members of the Congress should be invited to subscribe as founder members and that the subscriptions of all institutions and individuals who register for membership at the annual rate during 1958 should be deemed to secure membership from the time of registration until December 31st 1959.

24. It is recommended that the panel of Honorary Officers and Council of the Association should be nominated at a meeting of the Section on Nomenclature, and that this Section be empowered to elect such a panel as Council Designate of the Association, the said Council to assume full powers on authorisation of the Congress in Plenary Session.

25. It is recommended that the first Council, thus elected, be instructed to draw up the draft of a Constitution for the Association, and that this draft should be published or otherwise made available to all members of the Association before 30th June 1959, and that the adoption of the said Constitution shall be by a postal referendum to all members, closing on 31st December 1959.

Section C. PUBLICATIONS

26. The present official publications are issued for the Commission by the International Trust for Zoological Nomenclature, and consist of the *Bulletin*, the *Opinions*, and occasional volumes, such as the *Copenhagen Decisions* and the *Official Lists* and *Indexes* of Zoological Names.

27. Apart from financial considerations, the particular advantage of the present scheme is that all applications presented to the Commission, and most documents relating to them, are transmitted to the Commission and the public simultaneously by publication in the *Bulletin*. Comments on previous published applications are issued serially in the *Bulletin* as received, and are not, therefore, in immediate juxtaposition to the cases to which they refer. Both the applications themselves, and also comments relating to them, are, however, brought together after submission to the Commission, and are republished, either in full or in part, in the *Opinions*. Also included are any other documents that have been issued to the Commission in mimeographed form, the *Opinion* as finally rendered by the Commission, and the state of votes recorded by the Commissioners.

28. The disadvantage of the existing scheme as outlined in the previous paragraph is that most of the documents in question are printed twice, first in the *Bulletin*, then in the *Opinions*. A secondary disadvantage is that the rather full documentation considered necessary for submission to the Commission frequently obscures, by its comprehensiveness, the fundamental nomenclatural point at issue.

29. The *Interim Committee*, conscious as they are of both the advantages and disadvantages of the present scheme, and taking into consideration also the financial issues involved, make the following recommendations. It is suggested that the regular publications should remain two in number, but that the duplication, in verbatim form, of lengthy passages in both *Bulletin* and *Opinion* should cease. The full documentation necessary for submission to the Commission should, according to these recommendations, in future be published in a form which will introduce separate pagination for each Application. Each application will be allotted a Case Number, and subsequent documents referring to this case will be published *seriatim* as received, and will conclude with the official *Opinion* rendered by the Commission, and a report on the voting recorded. The original application, all documents referring to it, and the final *Opinion* rendered will thus be paginated consecutively and bound up together. The *Opinions* thus rendered, and the documents on which they were based, will be gathered together in volumes of convenient size as are the *Opinions* at the moment. The price of the *Opinions* and supporting documents thus issued will be regulated by the cost of printing and distribution.

30. The second regular publication recommended by the Committee will be a journal, issued free to all members of the *International Association*, whose prime purposes will be :

(a) summaries of all applications and comments published in the *Opinions* series ;

(b) notices of cases pending under the Plenary Powers ;

(c) notices of nomenclatural proposals in accord with automatic procedures subject to challenge ;

(d) summaries of decisions rendered by the Commission ;

(e) general articles on nomenclature as space permits ;

(f) a list of all current issues of the *Opinions* series, together with their individual price.

In this way any zoologist interested in a case summarised in the journal will be able to apply for the fuller documentation available in the *Opinions* series. The title of this journal will be a matter of consultation between the Association, and the International Trust.

31. The fact that the Committee recommend a summarised treatment of each case, as well as a more fully documented treatment, does not mean that they believe the present full documentation could not, if appropriately edited, be considerably shortened, with a consequent gain in clarity of exposition. It is with this in mind that the Committee recommend below the appointment of an officer charged with the duties of editor.

32. The Committee realise that, as at present so in the future, it will be necessary to arrange for the publication of special volumes, the most important of which will be the *Code* itself. It is recommended that one copy of the *Code* should be issued free to each member of the International Association on receipt of entrance fee and first subscription. Other special volumes will usually be issued at a price fixed to cover cost of publication and distribution.

33. The Committee has investigated with some care the possibility of reducing printing costs by adopting other techniques of printing than letterpress (" varityper " composition, offset lithography, photoscopic stencils, etc.). The Committee are able to report that there would be no significant saving in cost of these methods over letterpress if the total printing can be kept under £2,000 per annum. There would be a progressively greater saving if printing costs rose above this sum, but the work of supervising the various composition and printing processes would be considerable, and the quality of the finished article would be somewhat less attractive than if printed by letterpress. The Committee is therefore unable to recommend any method of printing other than letterpress. Comparative estimates have, however, been sought from printers in different countries. The Committee is pleased to report that the well-known firm of Brill of Leiden have offered terms somewhat below all others. It is, therefore, proposed that this firm should be accepted for the present as Printers to the Commission. The choice seems a particularly happy one in that one of the Committee's nominees for the post of Joint Secretary to the Commission, Dr. L. B. Holthuis, is also of Leiden.

34. All Commissioners should continue to receive the publications of both Trust and Association free of charge.

Section D. RE-ORGANISATION OF THE SECRETARIAT

35. The Copenhagen Plan calls for the re-allocation of the duties hitherto undertaken by the Honorary Secretary so that they should be spread, some to be carried out by an additional honorary officer (Editor) others (including financial matters) to be handled by the International Association through its officers and members. The Interim Committee have had the recommendations of the Copenhagen Plan constantly in mind throughout the inter-Congress period, and have surveyed the various possibilities presented by the changing circumstances. They stand firmly by the basic principle as accepted at Copenhagen, namely that the senior officers of the Secretariat should serve in an honorary capacity. Furthermore, they are of the opinion that a suitable choice of such officers will lead to the offer of rent-free accommodation.

36. They recommend one modification in the Plan as conceived at Copenhagen. They recommend that the two honorary executive officers, which the Copenhagen Plan suggested should carry the titles of Honorary

Secretary and Honorary Editor respectively, should rather be entitled Joint Honorary Secretaries. This change in title will give to the Secretariat, it is suggested, a greater flexibility.

37. The Committee has also spent some time and effort in evaluating the effect that various changes in the location of the Secretariat might have in general policy. They are of the opinion that it is essential that the main expenses (i.e., those of printing and publication) should be incurred in a " soft currency " area, and it is partly for this reason that they have recommended the Dutch firm of Brill as printers. But they are also sensible of the fact that a majority of authors on Systematic Zoology are situate in North America, and that the language most frequently adopted for articles and applications concerned with zoological nomenclature is English.

38. For these reasons, the Committee recommends that one of the Joint Secretaries should be a continental European, and that the other should be a native of an English-speaking country, preferably on the North American continent.

39. It is with pleasure that they announce that the following two gentlemen have agreed to accept nomination as Joint Honorary Secretaries to the Commission :—

Dr. L. B. Holthuis, of the Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands ;

Mr. Curtis W. Sabrosky, of the U.S. Department of Agriculture, Washington, D.C., U.S.A.

40. They are further able to announce that both of these nominees have approached the institutions of which they are members, and that these institutions have agreed, if the nominees are elected to office, to make available the necessary accommodation free of charge and to permit the officers in question to spend some part of their official time on the business of the Commission.

41. It is considered essential that funds should be made available to allow the employment of paid clerical help additional to whatever may be forthcoming as a result of the generosity of the institutions concerned, and this is duly budgetted for in the following section of these Recommendations.

Section E. BUDGET

42. The International Trust for Zoological Nomenclature was incorporated in 1947 for the conduct of financial affairs on behalf of the Commission. There

were initially seven members of the Trust. The Trust is managed by a Committee, who may confer any or all of their powers on an officer termed the Managing Director. There were initially four members of the Committee, and Mr. Francis Hemming was appointed Managing Director. New members of the Trust " being members of the Commission or persons approved by the Commission " may be admitted by the Committee.

43. The conduct of the Trust is controlled in some detail under English law according to the " Memorandum of Association " and the " Articles of Association ". There is provision in the Memorandum for collaboration between the Trust and such an organisation as the proposed International Association of Zoological Nomenclature. The following is an extract from the list of " objects for which the Trust is established " as noted in the Memorandum of Association : " To collaborate with similar societies, bodies and persons in the United Kingdom and in other countries with the object of securing the comparative study, upon lines and by methods similar to those of the Trust, of problems relating to zoological nomenclature common to the Trust, and to such other societies, bodies and persons and the Commission ".

44. The Interim Committee is of the opinion that the most happy and effective arrangement for the future would result from the close collaboration of Trust and Association. Such a collaboration could more easily be effected should the Trust decide to recruit to its membership some of those who were also Council-members of the Association.

45. If the Committee of the Trust supported such collaboration, the future income of the International Commission would primarily be derived from two sources :

(a) sale of publications issued by the Trust ;

(b) subscriptions to the International Association.

46. The income of the Trust in 1956 was mainly derived from two sources :

(a) the sale of *Opinions* (£7,235) ;

(b) the sale of the *Bulletin* (£3,703).

47. According to the publication policy recommended above in Section C, the material previously published in the *Bulletin* would in the future appear in the *Opinions* series. All new applications and all applications outstanding would be destined to appear once only in the *Opinions* series, and it is, therefore, anticipated that this source of income would continue as in the past, and would indeed probably increase if the output of the Commission increases as forecast by Mr. Hemming.

48. The Income derived from the sale of the *Bulletin* would be replaced by the collection of subscriptions by the International Association. In estimating the support likely to be given to such an Association, the Interim Committee has been guided by the support given to two organisations of a somewhat similar nature. The first is the International Association for Plant Taxonomy. The second is the Society of Systematic Zoology (of the United States of America).

49. The International Association for Plant Taxonomy was inaugurated on 10th July 1950, during the VIIth International Botanical Congress at Stockholm. 130 foundation members were enrolled and headquarters established in the Netherlands. On February 1st, 1952, membership had increased to 435, coming from 38 countries. By 1952 this had increased to 600 members from 50 countries, and by 1954 to 700 members from 58 countries. The I.A.P.T. recognises the following classes of membership :

Individual :	*Annual Subscription*
Regular	£1 1s. ($3.00)
Associate	7s. ($1.00)
Institutional :	
Supporting	£17 10s. ($50.00)
Regular	£8 15s. ($25.00)
Associate	£3 10s. ($10.00)

The Association publishes a Journal entitled " *Taxon* ", which all regular members receive free. Nine numbers, totalling 280 pp., were issued in 1957. It also publishes occasional volumes in a series known as *Regnum Vegetabile*, some of which are issued free to regular members, others made available at reduced prices. The 1952 edition of the *International Code of Botanical Nomenclature* was issued in this form free to regular members. The Committee has been informed that the subscription charged for *Associate* membership does not cover the cost of the publications issued.

50. The Society of Systematic Zoology is an American society with a membership of 1,200. The annual dues are $2.00 for membership, $4.00 additional for those members desiring to subscribe to the journal " *Systematic Zoology* ".

51. The Committee is encouraged to believe that the proposed *International Association for Zoological Nomenclature* will not receive less support than the similar *International Association for Plant Taxonomy*. The number of zoologists employed in systematic work is greater than the number of botanists similarly employed, and some idea of those potentially interested in zoological nomenclature can be gained by the membership of the Society of Systematic

Zoology in America. The Committee believe that this is an indication that it would not be unduly optimistic to forecast that the membership of the Association might reach 1,000 during the first three years, as many as 500 from America, and an additional 500 from the rest of the world.

52. In an attempt to estimate how many institutions would be prepared to support the Association as Institutional Members, members of the Committee have privately approached the librarians of certain institutions who at present subscribe to the *Bulletin.* As a result of these investigations the Committee believes that within the first three years the number of Institutional Members enrolled in the Association would exceed the present number of subscribers to the *Bulletin.* It is expected that some of those institutions that subscribe at present to the *Bulletin* but not to the *Opinions* will in future also subscribe to the *Opinions* series, as duplication is to cease.

53. From these considerations, the Committee forecasts that the income collected by the International Association should reach at least £1,400 by 1961.

54. Expenses of the new Secretariat will, if the recommendations are approved, be divided into three categories :

office expenses ;
printing expenses ;
salaries.

55. Office expenses will include stationery and postage, and it is not anticipated that they will be less than £1,000, which is the approximate cost noted in the expenses of the Secretariat in 1956, less the amount expended on rented accommodation for storage of back-numbers of publications. This sum will be split between the Netherlands and America.

56. It is recommended that all printing be executed in the Netherlands. It is difficult to budget any exact figure without specifying the size of the journal to be issued. Purely for the purposes of this calculation, and without recommending any particular size of publication as appropriate, a monthly printing of 2,000 copies of a journal, each part consisting of 32 pp., would have cost about £875 in 1954. To this must be added £5,000 for the printing of the *Opinions* series (which cost £4,234 according to the figures given for 1956).

57. The printing of the *Code* would have to be subsidised to a certain extent. It is understood that considerable donations have already been subscribed for this purpose, and are held by the International Trust. The

entrance fee of 7s. payable to the International Association would help offset the free issue of the *Code* to new members. It is recommended that the *Code* should be offered to the public at £1 15s. ($5.00).

58. The salaries envisaged under the recommended scheme are confined to the purposes of providing clerical assistance for the two Joint Secretaries. The wages thus proposed amount to £715 ($2,000) for the United States and £480 for the Netherlands.

59. Summarising the expenses envisaged for 1959 are as follows :—

Office expenses	£1,000
Printing expenses	£5,875
Salaries	£1,200
Total	£8,075

60. The income expected during the first year (1959) can be estimated as :—

Sale of *Opinions* series (including back-stocks of *Opinions* and *Bulletins*)	£7,000
Subscriptions to the Association	£1,075
Total	£8,075

61. That part of the expenses expected to be incurred in the United States must be met from income collected in the United States. It will, therefore, be necessary to appoint a Regional Treasurer for the United States, which officer could most conveniently act on behalf of both the International Association and the International Trust.

62. The appointment by the Trust of Mr. R. V. Melville as Assistant Secretary to the Commission for one year, from 1st May 1958 to 1st May 1959, should facilitate the smooth transfer of the Secretariat from its present offices to the new offices that are now recommended for Leiden and Washington.

63. The Trust as constituted must have its Registered Office situate in England. If the Secretariat were to move out of England, it might be advisable for the Trust to move also. Although this is not possible in the terms thus quoted owing to legal regulations, the Committee has been given to understand by Mr. Hemming that the Trust could hand over all its assets and responsibilities to a similar organisation in another country, providing the Trust was satisfied that such another organisation had legal safeguards comparable to those imposed on the Trust itself.

ANNEXE I TO THE APPENDIX TO THE INTERIM COMMITTEE'S REPORT

Report of the Committee appointed under the Chairmanship of Professor Spärck to consider ways and means of improving the financial position of the International Trust for Zoological Nomenclature

Copenhagen, 10*th August* 1953

In view of the announced intention of the Honorary Secretary to retire from his position before the next Congress, the Committee considers it absolutely essential to face up to the difficult task of finding a replacement and also to explore the possibilities of placing the entire Secretariat of the Commission on a sound financial basis which will require less personal sacrifice on the part of the Secretary. This the Committee understands to be the problem referred to it by the Rt. Hon. Walter Elliot, Chairman of the International Trust, by Mr. Francis Hemming, Honorary Secretary and Managing Director of the Trust, and by Prof. Dr. R. Spärck, President of the International Congress of Zoology.

In approaching the problem, the Committee recognizes that it is extremely unlikely that a new secretary can be found who possesses the remarkable combination of characteristics which have made the present secretary's regime so productive and successful. Even if a new man of this calibre were found, it would be undesirable, as it is even now unfair, to impose such an enormous load on one person. The Committee therefore believes that it is essential to realize that in the future the new Secretariat must inevitably operate on quite a different scale and in quite a different manner than at present.

With these considerations in mind, the Committee respectfully offers a plan for consideration by the Permanent Committee of the Congress. The plan is set up in three stages, recognizing that a suitable transition period would be an essential feature in any scheme.

FIRST STAGE : The first stage must clearly be to appoint the necessary new officers, preferably before the end of the present Congress. These, the Committee feels, should be two in number, an INTERIM SECRETARY-DESIGNATE and an EDITOR-DESIGNATE. The Permanent Committee will perhaps desire that such officers should be elected to the International Commission if not already members, and, if they concur, they will no doubt approach the International Commission with a view to securing their election forthwith. If the right men can be found and appointed at this time and if, in addition, they have participated in the Colloquium at Copenhagen, the Committee feels that important advantages would result, as follows : (a) the

Permanent Committee of the Congress and the International Commission would have ensured continuity of the Secretariat during the next inter-congress period ; (b) the Interim Secretary-Designate would have the official authority and blessing of the Congress ; and (c) the Interim Secretary-Designate would have the enormous advantage of advice and consultation with the present Honorary Secretary during the remaining period of service of the latter. The first of the duties of the Interim Secretary-Designate would be to plan and organize the future of the Secretariat along the lines suggested below.

SECOND STAGE : The second stage would commence immediately at the close of the Congress and would be terminated as soon as the resignation of the present Honorary Secretary becomes effective. Mr. Hemming has requested that arrangements be made to permit him to devote his attention to completing the proceedings of the Copenhagen Colloquium, to assembling the materials to be used as a basis for President Bradley's consolidation of the new Rules, and to finishing the individual cases which are now before the Commission. The Committee feels that this stage will perhaps be the most important in the entire history of the Commission. Therefore every effort should be made to facilitate the important work of the Honorary Secretary. To this end it is proposed that funds be solicited for the immediate employment of additional clerical assistance for the present Secretariat.

THIRD STAGE : The Committee believes it essential that the inauguration of the new plan coincide with the publication of the new Rules. This is all-important because of the opportunity presented at that time to capitalize upon the increased interest in nomenclature which will surely occur. Steps should be taken to see if such a timetable would be acceptable to Mr. Hemming, and, if so, the third stage would start with the assumption of full responsibility by the Interim Secretary and the Editor. It is suggested that, in view of the necessity of putting into operation a new and as yet untried plan, this third stage be regarded as terminated at the next Congress, when the whole arrangement will be subject to review.

The plan that the Committee proposes should be put into force during this period is as follows :—

1. A new financial structure shall be organized on the basis of an international society which would be organized for the study of zoological nomenclature in general and for support of the International Commission in particular. The detailed organization and promotion of the new society would be a primary duty of the new Secretary—working in co-operation with an interim sub-committee of the Congress (see below), with existing scientific societies and institutions, and with individual zoologists throughout the world. It is suggested that the new society consist of subscribing members, and various categories of contributors including private and public institutions and perhaps also governments. All subscribers would receive the *Bulletin of Zoological Nomenclature* at no extra cost and other publications of the Commission at a reduced rate.

The new Revised Rules would also be issued free to all members during the first year. The possibility of affiliation with other societies which already have a large membership is considered advantageous and desirable.

2. It is clear that during the Second Stage, pending the adoption of the full plan, it will be essential that the *Bulletin of Zoological Nomenclature* and the policy and structure of the International Trust for Zoological Nomenclature remain unchanged.

Since a substantial increase in income from publications cannot be anticipated in the immediate future, a grant-in-aid will be necessary for one or two years after the new scheme comes into operation. It is expected, however, that funds will become adequate as the new plan gathers way.

3. In the view of the Committee, it is absolutely essential to make reallocation of the arduous duties which are now carried out at great personal sacrifice by the present Honorary Secretary. Under the proposed scheme some of these duties, including financial matters, would be handled by the Society through its officers and members. It will be a matter for detailed consideration whether such a procedure will fit in with the present structure of the International Trust for Zoological Nomenclature. If not, alternative arrangements will have to be inaugurated during the Second Stage. The Committee inclines to the view that in any case the Interim Secretary-Designate should not hold conjointly the post of Honorary Managing Director of the Trust. If it were found desirable to dissolve the Trust, it would of course be necessary for the financial duties of the Managing Director to be delegated to some other appointment.

4. It is further proposed that general matters relating to publications be delegated to a publication committee of the Society, of which, of course, both the Interim Secretary and the Editor would be *ex officio* members. Although the Editor and Interim Secretary would always have to maintain closest co-operation, all routine matters in connection with publications would be handled by the Editor. In this way the secretariat would be relieved of an enormous amount of routine work.

ACTIVATION OF THE PLAN: Specifically, the Committee proposes that before the conclusion of the Copenhagen Congress

(1) both a Secretary-Designate and an Editor-Designate be appointed;

(2) an Interim Committee of the Congress be appointed to carry out the wishes of the Permanent Committee during this period of transition (it is suggested that a member of the Permanent Committee of the Congress be designated as chairman of this Interim Committee and that the Secretary-Designate be made Secretary of the Committee.

Other members should be selected with a view to the international character of the project, but the Interim Committee need not be large) ;

(3) the Interim Committee be authorised to approve such a plan as outlined above when the details have been worked out by the Interim-Secretary-Designate and the time has come for inauguration of the plan.

(4) If this scheme is approved by the Committee, it will of course be necessary to ensure that the International Trust for Zoological Nomenclature be formally notified of the fact.

(5) It is further recommended that either the Permanent Committee or the International Commission should solicit whatever authorities may be desirable to secure for the two persons appointed release from such part of their present duties as might be necessary.

ANNEXE 2 TO THE APPENDIX TO THE INTERIM COMMITTEE'S REPORT

Action taken by the International Commission on Zoological Nomenclature and communicated to Professor Spärck on 10th August 1953

The Commission agreed :

(1) at once to create the following additional Offices, namely :—
   (a) the Office of Interim Secretary-Designate ;
   (b) the Office of Interim Editor-Designate ;

(2) to allot to the foregoing Offices the duties specified in the Report of the Committee established by the Colloquium on Zoological Nomenclature on the recommendation of the Right Hon. Walter Elliot, Chairman of the International Trust for Zoological Nomenclature ;

(3) to elect forthwith to be members of the International Commission Mr. P. C. Sylvester-Bradley (*Sheffield University*) and Dr. L. B. Holthuis (*Rijksmuseum van Natuurlijke Historie, Leiden*) ;

(4) to appoint Mr. Sylvester-Bradley to the Office of Interim Secretary-Designate and Dr. Holthuis to the Office of Interim Editor-Designate ;

(5) to appoint the foregoing Officers to be Interim Secretary and Interim Editor respectively as from the date on which Mr. Francis Hemming relinquishes the Office of Secretary to the Commission ;

(6) to enlarge forthwith the membership of the Executive Committee of the Commission by the addition thereto of the Interim Secretary-Designate and the Interim Editor-Designate.

Annexe 3 to the Appendix to the Interim Committee's Report

Income and Expenditure of the Interim Committee

Income

	£	s.	d.
Proceeds of cheque for $50.00 received from Entomological Society of America ..	17	12	0
Proceeds of cheque for $50.00 received from Society of Systematic Zoology, and transferred via an A/c in America, less charges levied by American bank	16	11	0
Refund on unused cheques		9	2
Balance ..	£34	12	2

Expenditure

	£	s.	d.
Postages	5	7	10
Telephone		3	0
Stationery	13	19	8
Hire of typewriter	1	7	0
Typists' fees	3	10	8
Bank charges and cheques		13	0
Secretary's travelling expenses	2	16	0
Duplicating costs	5	7	0
Cash in hand	1	8	0
Balance ..	£34	12	2

DOCUMENT B

COMMENT ON THE REPORT OF THE INTERIM COMMITTEE ON THE SUBJECT OF THE FUTURE ORGANISATION OF THE WORK OF THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Statement dated 8th July 1958 prepared by the International Trust for Zoological Nomenclature

[*Communicated to the Colloquium for information by direction of the President of the Congress*]

The International Trust for Zoological Nomenclature has been invited by the President of the Congress to give some preliminary comments on the Report of the Interim Committee set up by Professor Spärck's Committee after the Copenhagen Congress, for consideration by the *Comité Permanent*, to whom the Report has been referred by the President.

2. The Trust, as the body in whom are vested the assets on which the work of the Commission depends, has a special responsibility to the Congress in regard to the financial and administrative support of the continuing activities of the Commission. The Trust has considered the Interim Committee's Report in the light of its knowledge of the problems of the work of the Commission and its experience over the last ten years.

3. The Trust recognises that the Interim Committee has been bound to work out details of the plan of organisation contemplated at Copenhagen. At the same time, the Trust must draw attention to the very considerable change in circumstances that has taken place since the Copenhagen Congress. At that time the position of the Secretariat of the International Commission was extremely precarious, being entirely dependent on the free services provided by the Honorary Secretary to the Commission. The work required from the Secretariat as the result of the growing activity of the International Commission was clearly more than an Honorary Secretary, however able and energetic, could provide unaided, and there seemed no prospect of securing the finance needed to obtain assistance. There seemed every prospect that, when Mr. Hemming was forced on any grounds to give up his duties as Secretary, the work of the Commission which had so greatly increased in activity under his regime would come to a standstill, and that it might be extremely difficult, especially in an inter-Congress period, to make any workable arrangements for its future conduct. It was for this reason that Mr. Hemming brought this matter up at the Copenhagen Congress—he was already 60 at the time—by announcing his intention to retire before the next Congress.

4. As it turned out, Mr. Hemming was able to continue his work on a whole-time but unpaid basis for nearly five more years, during which a complete transformation has been brought about both in the scale of operations and in the financial position of the Commission. This development made it both necessary and possible to engage a small qualified salaried staff, and resulted in an entirely unprecedented flow of decisions, the benefits of which to zoology and palaeontology are already obvious.

5. In place of the precarious financial situation reported by the Chairman of the Trust at Copenhagen, the Trust became possessed of an income and reserves sufficient to provide a reasonable secure financial backing for the operations of the Commission. When therefore the contingency of Mr. Hemming's retirement actually matured in April last, it was possible to avoid any interruption by engaging a whole-time salaried scientist and to ensure a smooth transfer of the work of the Secretariat to new accommodation.

6. The basis of the financial policy consisted in placing on sale the *Bulletin of Zoological Nomenclature* and the *Opinions* which are the tools by which the Commission carries out its work. A sufficient number of important institutions in different parts of the world have become regular subscribers to these publications to provide the resources needed for financing the Secretariat and for building up resources for the future.

7. The Trust appreciates that at first sight it would appear economical to rely on honorary staff, but it wishes to place on record its considered view that in modern conditions the volume of work which is at present undertaken and the rate at which decisions are promulgated could not possibly be maintained on such a basis. Still less could that be done if the responsibility were divided between two or more persons and if it were attempted to carry on the work in two or more places in different countries. It has indeed become obvious that efficient service to zoologists and palaeontologists in what are mixed judicial and scientific questions depends on the maintenance of a whole-time staff, limited in number but well qualified, and working with adequate office and library facilities. On a point of detail the Trust suggests that the necessity or otherwise for renting the modest accommodation should not be regarded as an important factor in the decision of principle. It feels bound to warn the Commission that the disruption of the present organisation and its replacement by a system of the kind discussed at Copenhagen could only result in the rapid return to the precarious situation of the past, when only a trickle of decisions could be obtained and then only after a long delay. Even under present conditions there are about 300 cases in various stages of progress in the Office of the Commission.

8. In brief, the Trust suggests that the situation has developed in an unexpectedly favourable way since the Copenhagen meeting, that the contingency which the Plan there discussed was designed to meet has been successfully met, and that it is unnecessary and would be unwise to make fundamental changes at the present time.

9. On the other hand, the Trust recognises the importance of many of the points raised in the Interim Committee's Report and agrees that they require careful consideration. While the primary object of its publications is to provide the Commission with the full documentation of all relevant aspects of the problems on which it has to reach decisions, it recognises that there may be some possibility of simplification in these publications and is fully prepared to study the problem of achieving this. If such a simplification can be achieved consistently with meeting the Commission's needs, this might result in some reduction in the cost of these publications.

10. The Trust also feels that its work might be helped if there were associated with it a small number of zoologists from various countries with whom it could consult from time to time on various matters and would gladly consider any arrangements to this end that may be proposed. In the actual composition of the staff, there is of course no reason why qualified individuals from any country should not be eligible for appointment if they wish to be considered.

HURCOMB

Chairman, International Trust for Zoological Nomenclature

8*th July* 1958

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URCHASED
16 SEP 1957

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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BULLETIN OF ZOOLOGICAL NOMENCLATURE

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THE AGENDA PAPER FOR THE DISCUSSIONS ON ZOOLOGICAL NOMENCLATURE TO BE HELD AT, AND IN CONNECTION WITH, THE FIFTEENTH INTERNATIONAL CONGRESS OF ZOOLOGY, LONDON, JULY 1958

(Note by the Secretary to the International Commission on Zoological Nomenclature)

By an arrangement made with Sir Gavin de Beer, the President of the Fifteenth International Congress of Zoology to be held in London in July 1958, the International Trust for Zoological Nomenclature has allotted two volumes of the *Bulletin of Zoological Nomenclature* to serve as Agenda Volumes for the Section on Nomenclature of the foregoing Congress. These Volumes will in addition constitute the Agenda Volumes for the discussions to be held in connection with the above Congress both by the International Commission on Zoological Nomenclature and by the Colloquium on Zoological Nomenclature which is being organised by the International Trust and which will hold its First Meeting on Wednesday, 9th July 1958, i.e. one week before the actual opening of the Congress. The two Agenda Volumes so to be provided are Volumes 14 and 15.

2. The first of the London Agenda Volumes (Volume 14) is being devoted to the draft of the English text of the *Règles Internationales de la Nomenclature Zoologique*, as amended by the Paris (1948) and Copenhagen (1953) Congresses, which has been prepared by Professor J. Chester Bradley.

3. In the present volume, which constitutes the second of the two London Agenda Volumes, there will be published (a) any comments which may be received on Professor Bradley's draft and (b) any proposals for the further improvement of the *Règles* which may be received. The International Trust has been notified by the President of the London Congress that it is his view that the harvesting of the reforms of the *Règles* decided upon by the Paris and Copenhagen Congresses should be treated by the Section on Nomenclature as having the first priority and should be dealt with by it in advance of any other item on the Agenda. The work of the Colloquium is accordingly being organised on this basis.

4. The method to be followed in the present volume for publishing documents included in the London Zoological Nomenclature Agenda Paper will follow generally that adopted by the Trust when in 1953 it published (in Volumes 8 and 10 of the *Bulletin of Zoological Nomenclature*) the corresponding Agenda Paper for the Copenhagen Congress. As on that occasion each subject will be allotted a Case Number and papers relating to that Case will be allotted consecutive Document Numbers. Thus for Case Number 1 the first of the papers submitted will be allotted the Document 1/1, the second, Document 1/2 and so on.

5. All the subjects on which communications have already been received have been allotted Case Numbers and the individual communications have been allotted Document Numbers in the appropriate series. Under this arrangement the first instalment of papers to be published in regard to any given Case will bear consecutive Document Numbers. Thereafter, documents relating to the Case in question will be published as and when they are received.

6. The method of publication described above, for which there is no alternative if documents are to be published as rapidly as possible, inevitably means that in the later stages it will not be possible to group Documents by reference to the Cases to which they belong. In order to overcome this difficulty, it is the intention of the Trust to issue on the eve of the meetings of the Colloquium and the Congress a consolidated statement in which the comments and suggested amendments published in the present volume up to that time will be grouped by reference to the Articles of the *Règles* to which they respectively refer.

FRANCIS HEMMING
Secretary to the International Commission on
Zoological Nomenclature
Managing Director and Secretary to the
International Trust for Zoological Nomenclature

19*th July* 1957

SECOND VOLUME OF DOCUMENTS PREPARED
AS AGENDA DOCUMENTS

FOR USE BY
THE COLLOQUIUM ON ZOOLOGICAL
NOMENCLATURE,
THE INTERNATIONAL COMMISSION ON
ZOOLOGICAL NOMENCLATURE,
AND
THE FIFTEENTH INTERNATIONAL CONGRESS
OF ZOOLOGY
AT
THE MEETINGS OF THOSE BODIES TO BE HELD
IN
LONDON IN JULY 1958

CASE No. 1

SUGGESTED NEW ARTICLE : PROPOSED RECOGNITION OF THE CONCEPT "PARATAXON" AND THE PROVISION OF RULES FOR THE NOMENCLATURE OF UNITS OF THIS CATEGORY

(Commission Reference : Z.N.(S.) 1056)

DOCUMENT 1/1

Note on procedure : The document reproduced below as Document 1/1 was originally submitted by Professor Moore and Mr. Sylvester-Bradley in the form of a request that the Commission should render a *Declaration* introducing and defining the expression "parataxon" and providing for the regulation of the nomenclature of units belonging to the category so recognised.

As Secretary, I then took the view that owing to the novelty and complexity of the proposals submitted, procedure by way of a *Declaration* would be inappropriate and that the proper course would be (a) for a decision to be deferred until the meeting of the Fifteenth International Congress of Zoology in London in 1958, (b) for the paper submitted to be placed on the London Agenda Paper, and (c) that, prior to the Congress, all possible steps should be taken to canvass opinion among interested specialists. This view was accepted by the applicants. It is under the foregoing agreement that the following paper is here placed on the London Agenda Paper. [intl'd F.H. 11th July 1957.]

Proposed insertion in the "Règles" of provisions recognizing "Parataxa" as a special category for the classification and nomenclature of discrete fragments or of life-stages of animals which are inadequate for identification of whole-animal taxa, with proposals of procedure for the nomenclature of "Parataxa"

By **RAYMOND C. MOORE**
(*University of Kansas, Lawrence, Kansas, U.S.A.*)

and

P. C. SYLVESTER-BRADLEY
(*University of Sheffield, England*)

The purpose of the present application is to draw to the attention of the International Commission on Zoological Nomenclature the serious confusion of nomenclature that exists as a result of the naming of certain fragmentary fossils which have been classified without regard to the whole-animals of which they form part. A previous decision of the Commission made at Paris which relates to the problems here presented recommended that names applied to fragments* should be treated as technical terms rather than as zoological names. Authors are now preparing manuscripts for the *Treatise on Invertebrate Paleontology* dealing with the taxonomy of fragmentary fossils comprised in groups assigned to the holothurians, crinoids, conodonts, coccoliths, ammonoids, scolecodonts and others. Some of the manuscripts already submitted employ divergent methods of dealing with the nomenclature of fragments, and need revision if they are to comply with the *Règles.* If they follow the recommendations made at Paris (1948),[1] they have to employ a terminology that falls outside the scope of zoological nomenclature altogether. Thus they are deprived of the protection, regulation, uniformity and stability that the *Règles* afford to the taxonomy of whole-animals. This leads to uncertainty in the application of the Law of Homonymy which affects every branch of the Animal Kingdom, and we regard it as a matter of prime importance that the nomenclatural position of these groups should be regularized. Editorial policy as it concerns the *Treatise on Invertebrate Paleontology* is loath to allow important sections of the text to employ nomenclature which clearly conflicts with provisions of the *Règles*, or which falls outside the scope of the *Règles.* We accordingly submit that the problems raised are of urgency and warrant immediate attention.

2. We have consulted Professor J. Chester Bradley on the preparation of this application, and he informs us that there is a possibility that the recognition of parataxa might benefit other branches of taxonomy than the paleontology of fragments. For example, the scheme might be of use in the special problems concerned with the recognition of collective groups of certain stages, of great medical importance, in the life-histories of parasites. Our application is therefore worded so that the establishment of parataxa can be varied to suit diverse circumstances. At the same time a procedure is recommended which limits the application of the provisions relating to parataxa to rigidly defined groups of animals specifically approved for the purpose by the Commission.

3. Special category of zoological entities comprising discrete fragments : Discrete parts of various kinds of animals, chiefly skeletal parts, occur commonly in nature ; more especially they are represented by abundant fossils in sedimentary strata of all geological ages from Cambrian to Recent. Examples are isolated coccoliths ; spicules of sponges, octocorals, and holothurians ; ossicles of crinoids, cystoids, blastoids, echinoids, and asterozoans ; annelid jaws (scolecodonts) ; radular elements and opercula of gastropods and cephalopods (aptychi) ; and the abundant fossils of unknown zoological affinities called conodonts. A large majority, if not all, of these bodies are usefully classifiable within the groups to which they belong, even though the genera and species of animals from which they were derived is almost universally unknown. Such discrete fragments of animals constitute a special category of zoological entities which, though classifiable in varying degrees of detail and precision, offers critical problems in nomenclature.

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* As used throughout this application, " fragment " is understood to refer to part (or according to the suggestion stated in para. 2 below, to a life stage) of an animal when such part is deemed wholly inadequate for identification of a whole-animal taxon.

[1] The decision here referred to was taken by the International Commission on Zoological Nomenclature at a meeting held on 26th July 1948 (1950, *Bull. zool. Nomencl.* **4** : 293—294), that Meeting being the Eleventh Meeting of its Session held in Paris concurrently with the Thirteenth International Congress of Zoology, 1948. Owing to the complexity of the issues involved it was later decided to defer the preparation of a *Declaration* giving effect to the foregoing decision until there had been an opportunity to consider in greater detail the problems at stake.

4. Importance of nomenclature for animal parts : There is little need for the classification and nomenclature of fragments when whole specimens of animals are available for study. This applies to virtually all work by neozoologists on living animals and may be accepted also for most work by paleozoologists on extinct animals because the fossils on which many thousand taxa have been recognized and named are judged adequate for discrimination of various genera and species of whole animals. In addition, there are multitudinous dissociated fragments of animals which are far from sufficient for identification of the whole animals that produced them and yet these are so distinctive in themselves as to have great usefulness for identifying the sedimentary strata containing them. These fragmentary paleontological materials are indispensable for correlations of many rock formations in the earth's crust and for aid in establishing a trustworthy geochronology of the post-Precambrian part of geological time. However, in order to make use of such fragments, they must be classified, named, described, figured, and recorded as to occurrence. When this is done, many prove to be invaluable. For example, the dissociated fossils called conodonts have been demonstrated to constitute the only reliable means for determining correlations and relative geological age of various strata containing these fragments. Other highly fragmental remains of animals, especially echinoderms are similarly useful, but so far have been little studied because no satisfactory means of naming them in accordance with zoological rules has been available. When suitable procedure is provided for applying names to discrete animal fragments without reference to the whole-animal species which they represent, this will encourage greatly the study of such fragments, making them useful in stratigraphical palaeontology.

5. Systems of dual nomenclature : The taxonomic arrangement adopted in by far the greater majority of fossils studied is exactly comparable to that which would have been proposed if whole animals had been available for study. In many cases, if a fragmental specimen is at first inadequate for the identification of the whole animal from which it was derived, evidence may accumulate later which will establish its identity. In these cases the normal operation of Art. 27 of the *Règles* (which states that the Law of Priority applies when any part of an animal is named before the animal itself) takes care of the nomenclatural situation. In a certain number of cases, however, the stratigraphical importance of the fragments far transcends their importance as biological entities. In these cases a dual nomenclature has grown up, one providing names for the fragments, the other for the whole animals. Such dual systems are contrary to the present provisions of the *Règles*, but they have great utilitarian value and are currently employed in the taxonomy of conodonts (see First Supplemental Application submitted herewith[2]), ammonoid aptychi (see Second Supplemental Application submitted herewith[3]), holothurian spicules (see Frizzell and Exline, 1955, *Bull. Univ. Missouri School Mines & Metallurgy*, No. 89) and, to a somewhat lesser extent, in a number of other groups. This application seeks to regularize the establishment of certain of these dual systems by the establishment of parataxa as a special category for the classification and nomenclature of the specified fragments. In a sense a parataxon is a taxonomic category, but, as Professor Chester Bradley has pointed out to us, in a zoologically more important sense it is outside of taxonomy. The study of parataxa might even be termed " parataxonomy ". Zoological taxonomy is a single system based on natural relationships into which, with varying degree of success, all animals can be fitted. It is just because fragments of the type here described cannot be fitted into that system that parataxa are called for. It might be argued that if these names cannot be applied in ordinary taxonomy, then they are better ignored ; to this there is the very forceful counter argument that it would be most confusing to have the same name applied to both ordinary taxa and to parataxa. Such homonymy must be avoided. The regulations we here recommend therefore suggest that for all purposes except those of the Law of Homonymy, parataxa should be regarded as not coordinate with corresponding whole animal taxa. To this extent they may be ignored by the taxonomist who is only concerned with zoological taxonomy.

6. Divergent methods of naming animal fragments : Scientific names which have been published for discrete animal parts of the sort defined in the preceding paragraphs comprise more than 2,000 binomina which consist of Latin or Latinized words with the initial letter of the first word capitalized and that of the second word not capitalized. They form binomina which follow exactly the prescribed pattern of zoological nomenclature applied to species. Some authors have sought to treat such isolated fragments of more or less undetermined taxonomic placement as if they were actual whole-animal taxa, construing nomenclature of them as strictly subject to the *Règles*, whereas others have sought to treat them apart from taxa recognized by the *Règles*.

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[2] The Document here referred to is reproduced as Document 1/2 Case No. 1 (pp. 14—34 of the present volume).

[3] The Document here referred to is reproduced as Document 1/3 of Case No. 1 (pp. 35—71 of the present volume).

At least three divergent methods of nomenclature as regards assumed status of the fragments are discernible, as follows :

(a) Binomina may be treated as names for whole-animals, even though based on one or another kind of individual parts, and accordingly, these are conceived to be strictly subject to all procedures and rules of zoological nomenclature. This is the method universally applied to conodonts up to the discovery of distinctively organized associations of conodonts called " assemblages " in 1934, and is the method still adopted by all those specialists who have not considered the nomenclature of assemblages. Over 1,500 specific binomina have currently been proposed for the discrete conodonts.

(b) Binomina may be classed as " technical terms " (*Bull. zool. Nomencl.* **4** : 294) and not as zoological names, thus being excluded by definition from application of any zoological rules. Efforts to employ nomenclature of this sort have been published for discrete fragments of crinoids (Moore, 1938, *Denison Univ. Bull., Jour. Sci. Lab.* 33), holothuroids (M. Deflandre-Rigaud, 1953, Classe des Holothurides. *Traité de Paléontologie*, ed. Piveteau ; Paris. **3** : 948–957), coccoliths (G. Deflandre, 1952. Sous-embranchement des Flagelles. *Traité de Paléontologie*, ed. Piveteau ; Paris. **1** : 99–130 ; E. Kamptner, 1948 (Coccolithen aus dem Torton des Inneralpinen) *Wiener Beckens. SitzBer.* Abt. I. Oester. **157** (No. 1—5) : 1–16, 2 pl.) and ammonoid aptychi (see Second Supplemental Application submitted herewith[4]).

(c) Binomina may be treated as " form " taxa, consisting of " form-genera " and " form-species ". This is the method in current use when classifying discrete conodonts by those authors who also differentiate " assemblages " (see First Supplemental Application submitted herewith[5]).

7. Objections to using the same procedure for the nomenclature of fragments as for whole-animals : It is evident that if scientific nomenclature of animal fragments is precisely governed by the rules which apply to the naming of whole animals, there is continuous risk of more or less serious confusion and instability of zoological nomenclature in groups that commonly are represented by abundant fragments. Among other provisions, the Law of Priority stipulates that " the oldest available name is retained when any part of the animal is named before the animal itself " (Art. 27a), and accordingly, at any time when a little-known name for a fragment is discovered to have priority over a much-used different name for the whole animal to which the fragment is found to belong, important disturbance of nomenclature may ensue. Unless the Plenary Powers of the Commission are invoked and exercised, not only must the name of the whole-animal taxon yield to that of the fragment, but the nomenclature of all other fragments associated generically with the concerned fragment becomes unsettled. It would be untrue to suggest that " generic " grouping of fragments is ever exactly equivalent to generic grouping of whole-animal species. Examples of the nomenclatural confusion resulting from this situation are cited in the immediate succeeding supplementary applications dealing with conodonts and ammonoid aptychi.

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[4] See Footnote 3.

[5] See Footnote 2.

8. Objections to nomenclature of fragments by use of technical terms : If parts of animals are given binominal or other sorts of names which expressly are rejected from the domain of zoological nomenclature and application of the *Règles*, the purposes of orderly classification and description of fragmentary remains of animals are almost certain to be defeated. Absence of coordination and regulation in this area can only lead to chaos. Rules of homonymy and synonymy no longer apply ; priority of publication has no significance and no author needs to take account of work done by others. Such a method of dealing with fragmentary remains of animals does not merit serious consideration unless some sort of mechanism for regulation outside of the zoological rules can be devised, and even then, it seems to us, more would be lost than gained. Also, it is pertinent to emphasize the point that in the realm of palaeozoology all specimens representing kinds of animal life are varyingly incomplete, so that in fact, discrimination between fossils considered adequate for classification and nomenclature under the *Règles* and those which must be excluded from the application of the *Règles* on the ground of fragmentary nature, is entirely subjective. It is on this account that we recommend that the provisions concerning parataxa should be limited to special groups of animals specifically defined for the purpose by the Commission.

9. Objections to the nomenclature of fragments as " form "-taxa : It has been suggested by one of us (Sylvester-Bradley, 1954, *J. Paleont.* 28 : 333–336) that " form "-taxa, analogous to the " form-genera " or " organ-genera " of the Botanical Code, could with advantage be used for the nomenclature and taxonomy of fragmentary fossils, and in fact this method is already in current use in the classification of conodonts (see succeeding supplemental application A for the recognition of conodonts as parataxa). The procedure, however, contravenes the *Règles* as they at present exist. Moreover, we feel that the terms " form-genera " and " organ-genera " would be ambiguous if applied to animal fragments, as concepts of " form " and " organ " are not precisely equivalent in botany and zoology. Therefore, we recommend that the Commission considers the introduction of a new term for taxonomic units composed exclusively of fragments of animals, and the term we propose is parataxon (" associate taxon ").

10. Procedure for nomenclature of parts of plants and whole plants in paleobotany : The problem of classifying and naming fossil plants is not different in kind from that encountered by paleozoologists in studying fossil remains of animals or by students of some Protista. Commonly, the various parts of a plant, such as leaves, impressions of bark, fruits, and roots, occur separately, but enough associated parts of some plants are found preserved as fossils to show characters of the whole plants. Paleobotanical rules take account of this situation by providing for separate categories of names, those for parts of plants being termed " form-species " and " form-genera " or " organ-species " and " organ-genera ". Names belonging to these categories are not applicable to those given to whole plants. The " form-species ", " form-genera ", " organ-species ", and " organ-genera " of paleobotanists are thus exactly equivalent to parataxa as here proposed for recognition by zoologists.

11. Procedure for nomenclature of animal fragments : The classification and nomenclature of animal fragments within the scope of the *Règles* are to be recommended, both because at present any exclusion is subjective and therefore ambiguous and because scientific studies of fragments need the guidance and protection furnished by universally accepted zoological rules. How can nomenclature of fragments under the *Règles* be provided without meeting insuperable difficulties ? The need is to provide a means of preventing (a) the invalidation of names applied to taxa of whole animals which are junior synonyms of parataxa ; and (b) the invalidation of parataxa as synonyms by the discovery that more than one parataxon belongs to a single whole-animal. The chief requisite is the recognition and segregation of parataxa, and their rigid exclusion from the category of commonly used taxa for whole-animals. Then, the rules governing all aspects of the nomenclature of taxa may be applied without any difference in mode or force to the category of parataxa, except that for the purposes of the Law of Priority, a wall should be conceived to separate nomenclature of whole-animal taxa from nomenclature of fragments defined as parataxa. Thus, without explicit sanction of the International Commission, a parataxon of fragments could not be transferred to the other side of the wall so as to be classed as a taxon of whole-animals, and the valid name of a whole animal never could be treated as a parataxon. Then nomenclature in the two realms would not be subject to instability by interference of one with the other. For the purposes of the Law of Homonymy, taxa and parataxa would be coordinate.

12. Procedure for determining which animal groups should be classified by reference to parataxa : The governing principle which alone can determine whether a part of an animal should be classified and named in terms of whole-animal taxa, or alternatively, in terms of part-animal parataxa, is the degree of adequacy of available specimens for determining what are judged to be diagnostic characters of a whole-animal taxon. Several sorts of animal parts present no problem when tested by this principle, for obviously they are fitted only for parataxa. Here belong all isolated skeletal elements of coccoliths, sponges, annelids, several classes of echinoderms, conodonts, and some parts of mollusks such as opercula and elements of the radulas of gastropods and aptychi of ammonoid cephalopods. Decision as to whether a particular group of animal fragments should be classified in taxa or parataxa must clearly, however, be removed from the realms of subjective judgment. It is therefore recommended :—

(1) that the parataxa system should only be recognized as validly applied to those animal-groups specifically authorized for that purpose by the Commission ; and

(2) that special procedures should be provided for disposing of questioned determination of the status as taxa or parataxa of certain names applied to animal fragments.

It will be necessary then for any taxonomist desirious of employing the parataxa system in cases not previously recognized to make application to the Commission for authorization called for in point (1) of the present paragraph. Decisions of the Commission given in such cases would not necessitate use of the Plenary Powers, but we consider that it would nevertheless be desirable to stipulate that voting on such application should not take place until a period of six months had elapsed after its publication in the *Bulletin of Zoological Nomenclature*[6]. This would allow time for the Commission to receive and consider any objections that might arise from other specialists in the same field. Referring to subpara. (2), it may happen that some groups classifiable in terms of parataxa contain fragments which by their unusually distinctive nature are considered certainly referable to a whole-animal species ; it may then be sought to admit the nomenclature of that part as the name of a whole-animal taxon. If such action seems desirable as concerns previously published names, it is suggested that the Plenary Powers be invoked. Also, if such action seems desirable to an author on introducing a new name, then the " challenge procedure " outlined at Copenhagen could be invoked, whereby the author would submit his proposal to the *Bulletin of Zoological Nomenclature* ; if no objections were raised to such a proposal during a two-year period following publication, the name would automatically be regarded as applicable to a whole-animal taxon ; if objections were received, the case would be decided by the Commission.

Recommendations :

13. In view of the facts and considerations which have been stated above, we now formally submit the following request :—

(1) that Article 27, subsection (a) should be modified by excluding parataxa by the addition of the phrase " except for parataxa " ;

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[6] This is already the normal Rule in the case of applications of all types published in the *Bulletin of Zoological Nomenclature.*

(2) that a new Article should be incorporated

(a) defining the expression "parataxon" in the following terms:

> "a parataxon is a taxonomic category comprising discrete fragments or life-stages of animals which, by decision of the Commission, are deemed unidentifiable in terms of the whole animals from which they were derived";

(b) stipulating the following conditions for the institution of parataxa:—

(i) "any zoologist desiring that classification and nomenclature of a particular group of animal fragments should be made in terms of parataxa must submit formal application therefor to the Secretary of the Commission; the Commission will proceed to vote on such applications only after a period of at least six months has elapsed after publication in the *Bulletin of Zoological Nomenclature*[7]";

(ii) "once the Commission has ruled that the classification of any group of animal fragments shall be in terms of parataxa, that ruling shall apply retroactively, as well as to future publication, irrespective of whether the author in question uses the term 'parataxa'";

(c) stipulating that nomenclature applied to taxa and parataxa should be mutually exclusive and independent for the purposes of the Law of Priority, but coordinate for the purposes of the Law of Homonymy, names belonging to one category not being transferable to the other.

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[7] See Footnote 6.

DOCUMENT 1/2

Note on procedure : The paper reproduced below as Document 1/2 was originally submitted to the Office of the Commission at a time (as explained in the Note on Procedure prefixed to Document 1/1 (page 5 above) the applicants contemplated the possibility of approval for their plan for the recognition of the category " parataxon " being given by the International Commission by way of a *Declaration*. The paper given below was prepared as the first of a series of applications for the issue by the International Commission as soon as the " parataxa " scheme had been approved, of directions applying that scheme to particular groups. The decision to postpone the " parataxa " scheme until the London Congress in 1958 made it impossible to achieve any progress with the proposals submitted in the present paper. That paper is, however, placed on the London Agenda Paper for the purpose of illustrating the type of application which might be expected to be received by the Commission if the principal " parataxa " scheme were to be approved. [Under the established procedure of the Commission proposals relating to individual names are not considered by it at meetings held during Congresses, it having been found more satisfactory that such cases should be studied in detail by the full Commission under the normal Three-Month Rule. Accordingly, it is not proposed that the detailed recommendations in regard to individual names contained in this paper shall be considered at the London Meeting. The papers will, however, be placed before the Commission for decision under the normal procedure as soon as possible after the close of that Meeting. [intl'd F.H. 11th July 1957.]

First supplemental application : application for a ruling of the International Commission directing that the classification and nomenclature of discrete conodonts be in terms of " parataxa "

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and

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(Commission Reference : Z.N.(S.) 1056)

The purpose of this application is to remove existing instability and confusion of nomenclature relating to the fossils of unknown affinity termed conodonts and to promote continuity and universality of nomenclature as applied to natural assemblages of these fossils representing whole-animal taxa on the one hand, and as applied to discrete conodonts not identifiable in terms of whole-animal taxa on the other. Supplemental to a preceding application of general scope[1] which calls for recognition of nomenclatural units termed parataxa, a Ruling of the Commission is sought directing that the classification and nomenclature of discrete conodonts are to be in terms of parataxa, including both already described forms and those which may be described in future.

2. **Need for recognition of conodont parataxa :** The fragmentary fossils consisting of toothlike structures called conodonts are widely distributed remains of animals which have been demonstrated to possess high value in stratigraphical paleontology. Prevailingly they occur in isolated manner, different kinds (more than 1,800) being described and named as genera and species. However, at least 250 natural assemblages have been claimed to indicate associations belonging to individual animals of unknown affinity, and these too have been described as different genera and species and given names distinct from those attached to their discrete parts. The elements of such assemblages not only are found to be constant in composition (except for adventitious incompleteness of some specimens) but many show the parts preserved in oriented relationship to one another. As is made evident in the following paragraphs, the system of nomenclature currently adopted runs counter to the present *Règles* ; it is thus illegal and as a consequence unstable. This may be avoided in a manner which would advance continuity with maintenance of the present system of nomenclature by defining genera and species of discrete conodonts as parataxa, while recognizing certain assemblages of them as whole-animal taxa.

3. **Subjective nature of recognition of " assemblages " :** The recognition of an assemblage of discrete conodont parts as representative of a single animal is subjective. The evidence presented for many of those described is regarded as conclusive by a majority of authors, but a few of the assemblages described are now thought to be random segregations, perhaps of faecal nature. It is clearly desirable to remove subjective elements from application of the *Règles*, and it is suggested that this be achieved by adopting wording which will make it clear that names applied to assemblages of conodonts, presumed by the author to be representative of single animals, are not available as names for parataxa.

4. **Nomenclature of described assemblages :** Eight generic names have been proposed to designate assemblages of conodonts presumed by their authors to represent single animals. Each of these assemblages is composed

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[1] The application here referred has been placed on the London Agenda Paper as Document 1/1 (pp. 5—13 of the present volume).

of discrete parts, some assemblages being identified with genera and some with species established previously on the basis of discrete parts. The nomenclatural position of the type species of each nominal genus of assemblages is set out in Table 1 and further discussed in the following paragraphs.[2] Each assemblage contains parts assigned to more than one genus of discrete conodonts. On the other hand, several genera of discrete conodonts contain species assigned to more than one genus of assemblages.

5. The generic name *Duboisella* Rhodes (1952b), with type species *D. typica*, was proposed for 13 assemblages of conodonts of Pennsylvanian age from Illinois. The components of the assemblages were identified by Rhodes with five previously named discrete conodont species, each referred to a different previously named genus. By the application of Article 27, these five species should be placed in synonymy with *D. typica*, and the assemblage should be known by the earliest name applicable.

(a) The generic names in question are:

Prioniodus Pander, 1856;
Hibbardella Bassler, 1925;
Lonchodina Bassler, 1925;
Idioprioniodus Gunnell, 1933;
Metalonchodina Branson and Mehl, 1941;
Duboisella Rhodes, 1952.

The type species of *Prioniodus* is known only from the Lower Ordovician of Europe, and is most unlikely to represent a whole animal congeneric with *Duboisella*, which is from the Pennsylvanian of North America. Similarly, the type species of *Hibbardella* and *Lonchodina* are both of late Devonian age, and unlikely to be congeneric with *Duboisella*. The type species of *Idioprioniodus* (*I. typus*), however, is identified by Rhodes[*] as a member of the assemblage named *Duboisella typica*, and on this basis the name takes priority over *Duboisella*. The type species of *Metalonchodina* is also identified by Rhodes with a member of *Duboisella typica*. *Metalonchondina* must therefore be regarded as another junior subjective synonym of *Idioprioniodus*.

(b) By Article 27, the specific name *typica*, as used by Rhodes in the combination *Duboisella typica*, must be replaced by its earliest synonym.

The six specific names in question are:

bidentata Gunnell, 1931 (*Metalonchodina*);
clarki Gunnell, 1931 (*Lonchodina*);

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[2] See page 29.

[*] Rhodes regards *Idioprioniodus*, when used for designation of discrete conodonts, as synonymous with the genus *Ligonodina* Bassler (1925), whose type species is Devonian, although this synonymization would not be likely to apply to the genus when used to design assemblage .

conjunctus Gunnell, 1931 (*Prioniodus*);
subacoda Gunnell, 1931 (*Hibbardella*);
typus Gunnell, 1933 (*Idioprioniodus*);
typica Rhodes, 1952 (*Duboisella*).

Of the four names proposed by Gunnell in 1931, *subacoda* is only conditionally recognized by Rhodes as part of the assemblage in question ("*Hibbardella* cf. sub *acoda*"), but any one of the other three should, by application of the present *Règles*, be chosen as the senior name to designate the assemblage, which should therefore be known either as *Idioprioniodus bidentata*, *I. clarki*, or *I. conjunctus*, but not (according to the present *Règles*) as *Duboisella typica*.

(c) In addition to the type species, four other species based on discrete conodonts have been assigned to *Metalonchodina* as follows:

M. acutirostris Mehl and Thomas, 1947, Miss., N.Am.;
M. alternata Mehl and Thomas, 1947, Miss., N.Am.;
M. deflecta Youngquist and Heezen, 1948, Penn., N.Am.;
M. tenora Ellison, 1941, Penn., N. Am.

Possibly some of these species may belong to whole-animals congeneric with *Duboisella typica*; probably others do not. It is impossible to place a discrete conodont in such a whole-animal taxon unless it is recognizable as specifically identical with a form found in a natural assemblage. These natural assemblages are rare, only about 250 having been so far described, compared to the hundreds of thousands of discrete conodont fragments known. It is not very likely, therefore, that it will ever be possible to identify the four named species of *Metalonchodina* with whole-animal taxa. What name should be used to designate them meanwhile? The most convenient and the most logical method is to continue referring them to *Metalonchodina*, but this would only be possible, according to the *Règles*, if that name were declared to be a parataxon.

(d) Two nominal species in addition to the type species have been assigned to *Idioprioniodus*. These are not necessarily derived from an assemblage congeneric with *Duboisella typica*, but could be referred to the discrete genus *Ligonodina* with which *Idioprioniodus* has been previously identified.

6. Another case which raises problems of the same kind as *Duboisella* is that of the genus *Scottognathus* Rhodes (1953), also named for a species based on conodont assemblages. The type species is *Scottella typica* Rhodes (1952) which is type species of *Scottella* Rhodes (1952) (*non* Enderlein, 1910). and is based on 132 assemblages from Pennsylvanian strata of Illinois and Kentucky. Rhodes recognizes four " components ", each consisting of one or more pairs of conodonts, and each previously named as discrete conodonts. Actually Rhodes lists, as synonyms, a very large number of species of discrete conodont under each " component ", but the only names that need concern us here are the generic and specific names listed below.

(a) The six generic names in question in this case are :

Hindeodella Bassler, 1925 ;
Synprioniodina Bassler, 1925 :
Idiognathodus Gunnell, 1931 ;
Streptognathodus Stauffer and Plummer, 1932 ;
Ozarkodina Branson and Mehl, 1933 ;
Scottognathus Rhodes, 1953.

As in the case of *Duboisella*, it is only possible to maintain that generic identity exists between a genus based on discrete parts and a genus based on an assemblage if the type species of the discrete genus can be identified in an assemblage referred to the assemblage-genus. Neither the type species of *Hindeodella* nor that of *Synprioniodina* have been identified in assemblages. In any case, as species of each are known in more than one genus of assemblages, neither name is suitable as a replacement name for *Scottognathus*. Rhodes, on the other hand, identified the type species of both *Idiognathodus* and *Streptognathodus* as members of assemblages of *Scottognathus typica*. By application of Article 27, the genus should therefore be known as *Idiognathodus* ; *Streptognathodus* and *Scottognathus* become synonyms. *Ozarkodina* has a type species not yet identified in an assemblage, and remains, therefore, in the same position as *Hindeodella* and *Synprioniodina*.

(b) The six genera in question are :—

claviformis Gunnell, 1931 (*Idiognathodus*) ;
excelsus Stauffer and Plummer, 1932 (*Streptognathodus*) ;
delicatula Stauffer and Plummer, 1932 (*Ozarkodina*) ;
microdenta Ellison, 1941 (*Synprioniodina*) ;
delicatula Stauffer and Plummer, 1932 (*Hindeodella*) ;
typica Rhodes, 1952 (*Scottognathus*).

Thus, in this case, the prior name for the assemblage described by Rhodes as *Scottognathus typica* is *Idiognathodus claviformis* Gunnell, 1931. Fay (1952) records 64 nominal species of *Idiognathodus* and 43 of *Streptognathodus*. Of these 107 species, all but 31 have been listed by Rhodes as synonyms of forms found in such association with *Scottognathus typica* as to suggest that they have been derived from animals conspecific with this species. It is possible, but not certain, that these remaining 31 species represent animals congeneric with *Scottognathus typica*. Until such time as evidence indicates to which assemblage they belong it is difficult to see by what generic name they should be described under the present *Règles*. All doubt would be removed if they were classed as parataxa.

7. The case of the assemblage named *Illinella typica* Rhodes (1952) presents rather different problems. This species, based on 21 Pennsylvanian assemblages, contains components identified by Rhodes with three genera of discrete conodonts.

The three genera are :—

Lonchodus Pander, 1856 ;
Lonchodina Bassler, 1925 ;
Gondolella Stauffer and Plummer, 1932.

Of these three genera, only one contains individuals which have been identified with previously named species, and no type species of a previously named genus has been recognized in the assemblage. The species recognized is of *Gondolella*, and contains five synonyms, *G. curvata*, *G. magna*, *G. bella*, and *G. minuta*, all of Stauffer and Plummer (1932), and *G. neolobata* Ellison (1941). The proper name for *Illinella typica*, according to Article 27 could therefore be *Illinella curvata*, *I. magna*, *I. bella* or *I. minuta*. It does not seem likely that the type species of either *Lonchodus* or *Lonchodina* is congeneric with *Illinella typica*, for the type species of both are Devonian and other species of these genera have already been identified as members of assemblages ascribed to genera other than *Illinella* (see Table 1). On the other hand, it is possible that the type species of *Gondolella* (which is *G. elegantula* Stauffer and Plummer, 1932) is congeneric with *Illinella typica*, in which case, according to the present rules, it would have to take priority over *Illinella*. These uncertainties in nomenclature would be removed by the recognition of parataxa.

8. Each of the conodont assemblages described under the names *Lochriea* Scott (1942) and *Lewistownella* Scott (1942) consists of four groups of components which have been identified generically by Scott with names previously applied to discrete conodonts, but not specifically.

The genera concerned are as follows :

(a) *Lochriea montanaensis* Scott, 1942
  Prioniodus Pander, 1856 ;
  Hindeodella Bassler, 1925 ;
  Prioniodella Bassler, 1925 ;
  Spathognathodus Branson and Mehl, 1941.

(b) *Lewistownella agnewi* Scott, 1942
  Prioniodus Pander, 1856 ;
  Hindeodella Bassler, 1925 ;
  Cavusgnathus Harris and Hollingsworth, 1933 ;
  Subbryantodus Branson and Mehl, 1934.

Both these assemblages were described from the Heath formation of Montana (which is late Mississippian or early Pennsylvanian in age). It is extremely unlikely that either is congeneric with the whole animal represented by the discrete fragment from the Ordovician which is type species of *Prioniodus* ; nor is it very likely that the Upper Devonian-Lower Mississippian type species of the genus *Hindeodella* represents an animal congeneric with either of these assemblages. *Prioniodella* also has a Devonian type species and *Spathognathodus* a Silurian type species, so that these two genera can likewise be regarded as unlikely to be senior synonyms of *Lochriea*. *Cavusgnathus* and *Subbryantodus*, on the other hand, both have Mississippian type species which might possibly be congeneric with *Lewistownella*. As Scott states of *Lochriea*, then : " This Carboniferous animal most probably is not generic with the genotype on which the oldest form genus was originally based, and to call it by that generic name, and to reduce the other names to synonyms, as would be required by the International Rules, not only would be improper but would result in utter confusion ". Under operation of the present *Règles*, with present knowledge it is not possible to suggest which name is the most " proper " for either assemblage. The consequent ambiguity is solved in current practice by ignoring the application of the Law of Priority in these cases. By the recognition of parataxa all ambiguities would be removed and the current use of names sustained.

9. Attention now may be directed to assemblages which have been assigned generic names coinciding with those introduced for discrete conodonts. Two cases can be cited. These are described in paragraphs 10 and 11 below.

10. An assemblage described by Eichenberg (1930, 177–182) under the name " *Prioniodus hercynicus* n. sp." from the Carboniferous of Germany was based on 30 figured discrete fragments.

(a) Eichenberg compared his figures with many named species of discrete fragments, referred to the following genera, but none positively identified with any particular species:

Prioniodus Pander, 1856;
Lonchodus Pander, 1856;
Polygnathus Hinde, 1879;
Hindeodella Bassler, 1925;
Bryantodus Bassler, 1925;
Ancyrodella Ulrich and Bassler, 1926.

The genus *Prioniodus* Pander was based on discrete conodonts of Ordovician age, with *P. elegans* as type species. As a genus of discrete conodonts it is considered to range from Lower Ordovician to Lower Permian (Fay, 1952), and 171 nominal species have been assigned to it. Table 1 shows that species assigned to discrete fragments of this genus occur in more than one genus of assemblages, species of *Prioniodus* having been identified in fact, in four of the eight assemblages tabulated.

(b) Branson and Mehl (1941) identified some of the elements figured by Eichenberg with the following additional genera:

Gnathodus Pander, 1856;
Scaliognathus Branson and Mehl, 1941.

(c) Fay (1952) selected one of Eichenberg's 30 figures as " holotype " of *Prioniodus hercynicus*, and further synonymized this species with *Prioniodus undosus* Ulrich and Bassler, 1926. If Eichenberg's specimens all truly belong to one animal, it is doubtful if Fay's action, in the light of Article 27, is valid.

(d) Eichenberg's contention is subjective, and in this particular case the evidence that the assemblage has been derived from a single animal is not considered conclusive by all authors. However, if the present application concerning parataxa is accepted, the specific name *hercynicus* as published in the combination *Prioniodus hercynicus* (being stated by the author to be the name of an assemblage) would not be available for a parataxon, despite its having been published in combination with a generic name which we are recommending should be added to the *Official List of Generic Names in Zoology* as a parataxon. In such case, should any author wish to substantiate the identity of the single animal named by Eichenberg, it would be necessary for him to refer to a genus not based on discrete fragments.

11. A case closely similar to that of Eichenberg's assemblage named "*Prioniodus*" is that of a group of nine assemblages described by Schmidt (1934) under the name *Gnathodus integer* n. sp. The generic name *Gnathodus* was proposed by Pander in 1856 for discrete conodonts from Carboniferous rocks of Russia. The type species (by monotypy) is *G. mosquensis*. It is possible that *G. mosquensis* and *G. integer* represent whole animals that are congeneric and Schmidt was therefore following the provisions of Article 27 in placing his assemblages in this genus.

(a) Schmidt recognized in his assemblages discrete fragments representing the following genera:

Gnathodus Pander, 1856;
Lonchodus Pander, 1856;
Hindeodella Bassler, 1925;
Bryantodus Bassler, 1925.

(b) Rhodes (1952), from a study of Schmidt's figures, concluded that most of the discrete components had been misidentified. According to Rhodes the following genera of discrete conodonts were represented in the assemblages:

Hindeodella Bassler, 1925;
Synprioniodina Bassler, 1925;
Idiognathodus Gunnell, 1931, or
Streptognathodus Stauffer and Plummer, 1932;
Ozarkodina Branson and Mehl, 1933.

On this basis he believed that Schmidt's species should be referred to the genus he was then introducing with an assemblage as type species, *Scotella* Rhodes, 1952 (=*Scottognathus* Rhodes, 1953; see para. 5(2) above). However, a little later Rhodes (1953 : 612) changed his opinions, stating that he believed Schmidt had correctly identified the polygnathid elements as *Gnathodus*, and that as a consequence he did not believe the assemblage named *G. integer* to be congeneric with *Scottognathus typica*.

(c) If the name *Gnathodus* is, by a ruling of the Commission, to be restricted in use as the name of a paratoxon, a new generic name will be required for the assemblage which has *Gnathodus integer* Schmidt as type. We have communicated with Dr. Schmidt on this matter, and he recommends that in this case the name *Westfalicus* should be used for the genus of assemblages with *Gnathodus integer* as type. (*Westfalicus* Schmidt, gen. nov.; type-species *Gnathodus integer* Schmidt, 1934; diagnosis as given for *Gnathodus* Schmidt, 1934, pp. 79–84 ; gender, masculine ; derivation of name, from Westfalia, the province of Germany in which the type species was found ; distinction from *Scottognathus* Rhodes, 1953, the possession of a different polygnathid component.)

12. Nomenclatural confusion relating to Polygnathus : The earliest description of a presumed natural assemblage of conodonts was published by Hinde (1879, pp. 351–369), based on fossils obtained from Devonian rocks of New York, and to this assemblage he gave the new name *Polygnathus dubius* (*recte dubia*). Hinde's description and illustrations, however, partly were based on isolated conodonts which he considered to be equivalent to component elements of the assemblage. The same paper includes 19 other species referred to *Polygnathus*, all based on discrete conodonts, of which several now are considered by specialists to belong elsewhere. Miller (1889 : 520) designated *P. dubia* as the type species of *Polygnathus* and Bryant (1921 : 10–24) defined as lectotype of *P. dubia* one of the discrete conodonts figured by Hinde. This is an application of Article 27 of the *Règles* in reverse, for (as in the case of the restriction of the types of *Prioniodus hercynicus* Eichenberg and *Gnathodus integer* Schmidt mentioned above in paragraph 11 (c)), instead of the oldest name of a part applying to the whole, here the name of the whole (as intended by Hinde) is fixed to a part. In addition to *Polygnathus* as thus restricted, Branson and Mehl (1933 : 136–166) have identified among the constituents of *P. dubia* representatives of conodont genera named *Lonchodina*, *Hindeodella*, *Bryantodus* (all Bassler, 1925) and *Spathognathodus* (Branson and Mehl, 1941). Other authors have made other identifications as indicated in Table I. Shall the restriction in application of *Polygnathus* from the name for an assemblage to the name for a component be denied ? If so, the discrete conodonts which have come to be known as *Polygnathus*, including 179 described species, are left without a generic name. Also, because *Lonchodina* (59 species), *Hindeodella* (85 species), *Bryantodus* (154 species) and *Spathognathodus* (64 species) are names published long after 1879, these genera and their 362 contained species are threatened because as parts of the *Polygnathus* assemblage they might be classed as junior synonyms. It has been maintained that the assemblage described by Hinde is a fortuitious mechanical or faecal concentration. Under these circumstances it seems wise to request the Commission to regard the case of *Polygnathus* as an exception to the general provisions proposed here for the establishment of parataxa. We would recommend that the Commission should :

(a) reject (under the Plenary Powers if necessary) the name *Polygnathus dubia* Hinde, 1879, as an available name for an assemblage of conodonts presumed to represent a single animal ;

(b) issue a Ruling confirming the restriction of the lectotype of *Polygnathus dubia* to a specified component as discussed in the next subparagraph ;

(c) issue a Ruling that the generic name *Polygnathus* and the specific name *dubia* as used in combination with *Polygnathus* are to be regarded as parataxa.

13. The type species of Polygnathus : The position as regards the type-species of *Polygnathus* may be summarised as follows :—

(a) Hinde (1879 : 361–368) proposed the new genus *Polygnathus* " for an animal possessing numerous minute and variously formed Conodont teeth and similarly minute tuberculated plates grouped together, but of which the natural arrangement is not at present known. This meagre definition is all that is afforded by the single example of the genus met with, in which about twenty-four entire and fragmentary teeth and six plates have been crushed together in a a small patch of about one-fourth of an inch in diameter, in black shale ". The " single example " of twenty-four fragments is then described under the name *Polygnathus dubius* (*recte dubia*) and figured as pl. 16, figs. 6–18. Hinde immediately follows this description with the description of a further nineteen new species all assigned to *Polygnathus* (four questionably so). All these nineteen species were based on discrete conodonts. One was named *P. pennatus* (*recte pennata*).

(b) Miller (1889 : 520) listed *P. dubius* as type of *Polygnathus.*

(c) Bryant (1921 : 23) described a species of discrete conodont under the name *Polygnathus pennatus* Hinde, in the synonymy of which he included " *Polygnathus dubius*, G. J. Hinde (in part), . . . Plate XVI, fig. 17 ". In the description of the species he states : " If, for no other reason than convenience in classification, in the present state of our knowledge, I believe the genus *Polygnathus* should be restricted, so as to include only those tuberculated and rugose tritoral plates discovered for the first time with the type specimen and characteristic of it. These consist of leaf-shaped plates with a central rib or ridge which is produced beyond the tritoral margin into a stem-like flattened crest of pectinate teeth. *P. pennatus* is the smallest and one of the commonest of these forms."

(d) Ulrich and Bassler (1926 : 43) in discussing the genus *Polygnathus* make the following statement : " *Polygnathus* was founded by Hinde upon a group of plates and teeth occurring associated on the same slab of Rhinestreet shale from western New York which he believed to represent the remains of a single animal. In this assemblage no less than five genera as now understood were represented, and it would have been better to have discarded the genus. Since Bryant has redefined the genus and selected a genotype, *P. pennatus*, which he says is represented in Hinde's type assemblage of *P. dubius* in the edge view of the common Genundewa species the genus may be recognized in this restricted sense." In spite of Ulrich and Bassler's contention, Bryant's action could not, according to the present *Règles*, be " rigidly construed " as selecting a type species. However, Holmes (1928 : 17) lists *P. pennatus* as " genotype " with " *P. dubius* Hinde, 1879, part " in parenthesis.

(e) Roundy (1926 : 13) refers to Bryant's restriction of the genus *Polygnathus*, and states that : "As . . . many of the various teeth described and figured under the type species, *P. dubius*, have been referred to previously described genera, it is necessary to select the genotype. Of the forms figured by Hinde as *P. dubius* but three (Pl. 16, figs. 16, 17 & 18) could now remain under the genus. Bryant, however, says that figure 17 is only a differently oriented view of the form described by Hinde as *Polygnathus pennatus* and that figures 16 and 18 show different aspects of the form described as *P. cristatus*. I therefore propose that the genotype *Polygnathus dubius* Hinde be restricted to the specimen shown on his Plate 16 as figure 17. Hence *Polygnathus pennatus* Hinde becomes a synonym of *P. dubius*." Roundy reproduces Hinde's figure 17 under the name *Polygnathus dubius*.

(f) Branson and Mehl (1933 : 146) make the following statement : " Roundy calls Hinde's figure 17 of plate 16 the genotype of *Polygnathus*. Branson referred it to *P. pennata* after studying the types and comparing the specimens, but as both were in slabs the comparison was not entirely satisfactory. However, he took free specimens of *P. pennata* and compared them by placing them adjacent to the specimens on the slabs and thus verified his reference. The original *Polygnathus dubia*, which consisted of specimens of several genera . . . should not have one of its kinds used as genotype. Bryant used *P. pennata* as the genotype, and that seems the best way out of a bad situation. If that procedure is followed and it is recognized that Hinde's figure 16 of plate 16 is not identifiable as to species, only figure 18 of plate 16 remains to bear the name *P. dubia* and we are using that specimen as the type."

(g) If, as we recommend, the name *Polygnathus* is to be restricted to the nomenclature of a parataxon, it will clearly be necessary for the Commission to specify which species is to be regarded as type species, and which specimen is to be used in the interpretation of that species. It will be seen from the foregoing analysis of the situation that *P. dubia* has priority as type species, and that Hinde's Pl. 16, fig. 17 has priority as lectotype of that species. The specimen is preserved in the British Museum under the Catalogue Number A.4211. We recommend that the Commission should specify that the genus should be interpreted by references to this species and specimen.

(h) In the view of Bryant (1921), Roundy (1926), and Branson and Mehl (1941), the specimen selected as lectotype of *P. dubia* according to the recommendations is synonymous with the species described by Hinde in the same paper as *P. pennatus*. It now becomes necessary, therefore, to determine which of the two subjective synonyms, *P. pennata* or *P. dubia*, should have priority. Bryant (1921) synonymized the specimen subsequently selected as lectotype of *P. dubia* with *P. pennata*. Roundy (1926) on the other hand quotes *P. pennata* as a synonym of *P. dubia*, rather than vice versa. On the principle of " first reviser ", it is difficult to maintain that Bryant suppressed *P. dubia* in favour of *P. pennata*, for he listed other of Hinde's type-specimens of *P. dubia* in the synonymies of several other species. In view of this ambiguity we recommend that the Commission should rule that in the case of the type-specimens of *P. dubia* and *P. pennata* being considered members of one species, the former name is to have priority. This is in accord with page precedence.

14. Gender of the genera Polygnathus and Scottognathus : The genus *Polygnathus* Hinde (1879) was treated by him as masculine, and some subsequent authors have followed him in this practice. The word, however, is derived from the Greek γνάθος (jaw) which as a classical word has always been regarded as feminine. Of the 179 species listed under this genus by Fay (1952)47 are in masculine form, 71 in feminine form, and the remainder in a form equivalent for either gender. It is recommended that the genus be recorded as feminine in the *Official List*. The genus *Scottognathus*, also based on the same Greek word, was treated as masculine. The name is of rather recent introduction and has not yet been extensively quoted. We recommend it should also be recorded as feminine in the *Official List*.

15. It is not proposed that all the names discussed in this paper should at this time be entered on the *Official Lists* and *Official Indexes*, for many of them are subjective synonyms even within the category of parataxa. We believe that it is necessary, however, to enter on the *Lists* those names which we have discussed above in some detail. Three of these names, which we are proposing to enter on the *Official List* as parataxa, have become type-genera of families. These are POLYGNATHIDAE Bassler, 1925, PRIONIODONTIDAE Bassler, 1925 (as PRIONIODIDAE) and GNATHODONTIDAE Camp, Taylor and Welles, 1942. The last-cited name would be a junior homonym of GNATHODONTIDAE Huene, 1929, if that name were an available name. Huene (1929 : 49) introduced it for a family containing four genera of rhynchocephalian reptiles from the Triassic of South Africa. The name, however, is not nomenclaturally available, as it is not based on the stem of a type-genus. Huene credited the name to Broom (without date or bibliographic reference), but we have been unable to trace to Broom any use of the name in the form quoted by Huene. In a number af papers Broom referred to the " Gnathodonts " in the vernacular (e.g. 1906 : 598) and at least once (1906 : 599) used the name Gnathodontia as that of an Order.

16. In our view the present nomenclatural situation, which we have disclosed in the foregoing analysis, is undesirable, and prejudices the strict application of the *Règles* by leaving authors who work in this field with no alternatives other than to disregard the *Règles*, or disrupt nomenclature to an alarming extent. We therefore submit the following proposals to the Commission requesting that they direct :

(1) that the nomenclature of all categories based on types which in the opinions of the original authors, are discrete conodonts, shall be in terms of parataxa and as such shall be unavailable as names of taxa based on conodont assemblages ;

(2) that the names of all categories based on types which in the opinions of the original authors, are assemblages of conodonts derived from single animals, shall be unavailable for the designation of parataxa ;

(3) that, notwithstanding (2) above, the generic name *Polygnathus* Hinde, 1879 (: 359) (gender : feminine) (type species, by subsequent designaby Miller (1889 : 520) ; *Polygnathus dubius* Hinde, 1879) be placed on the *Official List of Generic Names in Zoology* as the name of a parataxon ;

(4) that the name *dubius* Hinde, 1879 (: 362–365), published in the combination *Polygnathus dubius* (Hinde, 1879) (type species of *Polygnathus* Hinde, 1879) is to be placed on the *Official List of Specific Names in Zoology* as the name of a parataxon, this species to be interpreted by the specimen figured by Hinde as Pl. 16, Fig. 17, now preserved in the British Museum (Natural History) under Catalogue Number A.4211, which specimen is to rank as lectotype ;

(5) that the following generic names, being introduced for assemblages of conodonts believed by their authors to represent single animals, are not available as names of parataxa, and are to be entered on the *Official List of Generic Names in Zoology* ;

Duboisella Rhodes, 1952 (: 895) (gender : feminine) (type-species, by original designation, *D. typica* Rhodes, 1952) ;

Scottognathus Rhodes, 1953 (: 612) (gender : feminine) (type-species, by original designation, *Scottella typica* Rhodes, 1952) ;
Illinella Rhodes, 1952 (: 898) (gender : feminine) (type-species, by original designation, *I. typica* Rhodes, 1952) ;
Lochriea Scott, 1942 (: 293) (gender : feminine) (type-species, by original designation, *L. montanaensis* Scott, 1942) ;
Lewistownella Scott, 1942 (: 299) (gender : feminine) (type-species, by original designation, *L. agnewi* Scott, 1942) ;
Westfalicus Schmidt, [? 1956] (gender : masculine) (type-species, by original designation, *Gnathodus integer* Schmidt, 1934).

(6) that the following specific names, type-species of the genera listed in para. (5), being based on assemblages of conodonts presumed by their authors to represent single animals, are not available as names of parataxa, and are to be entered on the *Official List of Generic Names in Zoology* :

typica Rhodes, 1952 (: 895), as published in the combination *Duboisella typica* (type-species of *Duboisella*) ;
typica Rhodes, 1952 (: 891), as published in the combination *Scottella typica* (type species of *Scottognathus* Rhodes, 1953) ;
typica Rhodes, 1952 (: 899), as published in the combination *Illinella typica* (type species of *Illinella*) ;
montanaensis Scott, 1942 (: 295), as published in the combination *Lochriea montanaensis* (type species of *Lochriea*) ;
agnewi Scott, 1942 (: 300), as published in the combination *Lewistownella agnewi* (type species of *Lewistownella*) ;
integer Schmidt, 1934 (: 77), as published in the combination *Gnathodus integer* (type species of *Westfalicus*).

(7) that the following generic names, being introduced as names of categories based on discrete conodonts, are to be entered as names of parataxa on the *Official List of Generic Names in Zoology* :

Prioniodus Pander, 1856 (: 29) (gender : masculine) (type species by subsequent designation by Miller, 1889 : 520, *P. elegans* Pander, 1856) ;
Gnathodus Pander, 1856 (: 33) (gender : masculine) (type species, by monotypy, *G. mosquensis* Pander, 1856) ;

(8) that the following specific names, having as type specimens discrete conodonts, are to be entered as parataxa on the *Official List of Specific Names in Zoology* :

elegans Pander, 1856 (: 5), as published in the combination *Prioniodus elegans* (type species of *Prioniodus* Pander, 1856) ;
mosquensis Pander, 1856 (: 34) as published in the combination *Gnathodus mosquensis* (type species of *Gnathodus* Pander, 1856) ;

(9) that the following family-group names, having as type genera conodonts classed as parataxa, are to be entered as parataxa on the *Official List of Family-Group Names in Zoology* :

POLYGNATHIDAE Bassler (1925 : 219) (type genus : *Polygnathus* Hinde, 1879) ;

PRIONIODONTIDAE (correction, first made herein, of PRIONIODIDAE) Bassler (1925 : 218) (type genus : *Prioniodus* Pander, 1856) ;

GNATHODONTIDAE Camp, Taylor & Welles (1942 : 525) (type genus : *Gnathodus* Pander, 1856) ;

(10) that the name *Scottella* Rhodes, 1952, a junior homonym of *Scottella* Enderlein, 1910, be entered on the *Official Index of Rejected and Invalid Generic Names in Zoology* ;

(11) that the names PRIONIODIDAE Bassler, 1925 (an Invalid Original Spelling of PRIONIODONTIDAE) and GNATHODONTIDAE Huene, 1929 (an unavailable name since not based on the stem of a type genus) be entered on the *Official Index of Rejected and Invalid Family-Group Names in Zoology.*

TABLE 1

Natural Conodont Assemblages and their Constituents

Names based on discrete conodonts with which constituents of assemblages have been identified [Type species of genera indicated by an asterisk (*)]	Names Applied to Conodont Assemblages							
	Duboisella typica Rhodes, 1952 (type sp., *Duboisella*, Penn., N. Am.)	*Scottognathus typica* (Rhodes), 1953 (type sp., *Scottognathus*, Penn., N. Am.)	*Illinella typica* Rhodes, 1952 (type sp., *Illinella*, Penn., N. Am.)	*Lochriea montanaensis* Scott, 1942 (type sp., *Lochriea*, Miss., N. Am.)	*Lewistownella agnewi* Scott, 1942 (type sp., *Lewistownella*, Miss., N. Am.)	*Prioniodus hercynicus* Eichenberg, 1930 (L. Carb., Europe)	*Westfalicus integer* (Schmidt), 1934 (U. Carb. Europe)	*Polygnathus dubia* Hinde, 1879 (type sp., *Polygnathus*, U. Dev., N. Am.)
	1	2	3	4	5	6	7	8
†*Idioprioniodus* Gunnell, 1933 (**I. typus* Gunnell, 1933, L. Penn-U. Penn., N. Am.) **I. typus* Gunnell, 1933	X	–	–	–	–	–	–	–
Metalonchodina Branson & Mehl, 1941 (**Prioniodus bidentatus* Gunnell, 1931, M. Penn., N. Am.) **M. bidentata* (Gunnell), 1931	X	–	–	–	–	–	–	–
Hibbardella Bassler, 1925 (**Prioniodus angulatus* Hinde, 1879, U. Dev., N. Am.) *H.* sp., cf. *H. subacoda* (Gunnell) 1931	X	–	–	–	–	–	–	–
Lonchodina Bassler, 1925 (**L. typicalis* Ulr. & B., 1926, U. Dev., N. Am.) *L. clarki* (Gunnell), 1931, L. Penn.-L. Perm., N. Am.	X	–	–	–	–	–	–	–
L. sp. [A], Rhodes, 1952	–	–	X	–	–	–	–	–
L. subsymmetrica Branson & Mehl, 1941 ...	–	–	–	–	–	–	–	‡[X]
Prioniodus Pander, 1856 (**P. elegans* Pander, 1856, L. Ord., Eu.), L. Ord.-U. Penn., Eu.-N. Am. *P. conjunctus* Gunnell, 1931, M. Penn	X	–	–	–	–	–	–	–
P. sp. [B], Scott, 1942	–	–	–	X	–	–	–	–
P. sp. [C], Scott, 1942	–	–	–	–	X	–	–	–
P. undosus Ulrich & Bassler, 1926 (*fide* Fay, 1952), U. Dev.-Miss., Eu.-N. Am.	–	–	–	–	–	X	–	–
P. radiatus Bryant, 1921	–	–	–	–	–	–	–	[X]

	1	2	3	4	5	6	7	8
Idiognathodus Gunnell, 1931 (**I. claviformis* Gunnell, 1931, Penn., N. Am.								
**I. claviformis* Gunnell, 1931	–	X	–	–	–	–	–	–
I. sp. [D], Rhodes, 1952	–	–	–	–	–	–	[X]	–
Streptognathodus Stauffer & Plummer, 1932 (**S. excellsus* S. & P., 1932, M. Penn.-U. Penn., N. Am.)								
**S. excellsus* S. & P., 1932	–	X	–	–	–	–	–	–
Ozarkodina Branson & Mehl, 1933a (**O. typica* B. & M., 1933, M. Sil., N. Am.)								
O. delicatula (S. & P.), 1932, Dev.-Perm., N. Am.	–	X	–	–	–	–	–	–
O. sp. [E], Rhodes, 1952	–	–	–	–	–	–	[X]	–
Synprioniodina Bassler, 1925 (**S. alternata* U. & B., 1926, U. Dev.-L. Perm., N. Am.)								
S. microdenta Ellison, 1941, Penn., N. Am. ...	–	X	–	–	–	–	–	–
S. sp. [F], Rhodes, 1952	–	–	–	–	–	–	[X]	–
Hindeodella Bassler, 1925 (**H. subtilis* U. & B., 1926, U. Dev.-L. Miss., N. Am.)								
H. delicutala Stauffer & Plummer, 1932, M. Penn.-U. Penn., N. Am.	–	X	–	–	–	–	–	–
H. sp. [G] Scott, 1942	–	–	–	X	–	–	–	–
H. sp. [H] Scott, 1942	–	–	–	–	X	–	–	–
H. sp. [I] Eichenberg, 1930	–	–	–	–	–	X	–	–
H. sp. [J] Schmidt, 1934	–	–	–	–	–	–	X	–
H. acicularis Branson & Mehl, 1933b	–	–	–	–	–	–	–	[X]
Gondolella Stauffer & Plummer, 1932 (**G. elegantula* S. & P., 1932, Penn., N. Am.)								
G. curvata S. & P., 1932, M. Penn., N. Am. ...	–	–	X	–	–	–	–	–
Lonchodus Pander, 1856 (**Centrodus simplex* Pander, 1856, Dev.-L. Carb., Eu.-N. Am.)								
**L. simplex* (Pander) *fide* Roundy, 1926 ...	–	–	–	–	–	–	–	[X]
L. sp. [K] Rhodes, 1952	–	–	X	–	–	–	–	–
L. sp. [L] Eichenberg, 1930	–	–	–	–	–	X	–	–
L. sp. [M] Schmidt, 1934	–	–	–	–	–	–	X	–
L. lineatus Pander, *fide* Roundy, 1926	–	–	–	–	–	–	–	[X]
Spathognathodus Branson & Mehl, 1941 (**Spathodus primus* B. & M., 1933a, Sil., N. Am.)								
S. sp. [N] Scott, 1942	–	–	–	X	–	–	–	–
S. sp. [O] Branson & Mehl, 1933	–	–	–	–	–	–	–	[X]
Prioniodella Bassler, 1925 (**P. normalis* U. & B., 1926, U. Dev., N. Am.)								
P. sp. [P] Scott, 1942	–	–	–	X	–	–	–	–

	1	2	3	4	5	6	7	8
Subbryantodus Branson & Mehl, 1934 (**S. arcuatus* B. & M., 1934, L. Miss., N. Am.) *S.* sp. [Q] Scott, 1942	–	–	–	–	X	–	–	–
Cavusgnathus Harris & Hollingsworth, 1933 (*C. *altus* H. & H., U. Miss., N. Am.) *C.* sp. [R] Scott, 1942	–	–	–	–	X	–	–	–
Gnathodus Pander, 1856 (**G. mosquensis* Pander, 1856, Carb., Eu. *G.* sp. [S] Branson & Mehl, 1941	–	–	–	–	–	[X]	–	–
G. sp. [T] Schmidt, 1934	–	–	–	–	–	–	X	–
Bryantodus Bassler, 1925 (**B. typicus* U. & B., 1926, U. Dev., N. Am.) *B.* sp. [U] Eichenberg, 1930	–	–	–	–	–	[X]	–	–
B. sp. [V] Schmidt, 1934	–	–	–	–	–	–	X	–
B. immersus Branson & Mehl, 1933c	–	–	–	–	–	–	–	[X]
Ancyrodella Ulrich & Bassler, 1926 (**A. nodosa* U. & B., 1926, U. Dev., N. Am.) *A.* sp. [W] Eichenberg, 1930	–	–	–	–	–	X	–	–
Polygnathus Hinde, 1879 (**P. dubia* Hinde, 1879, U. Dev., N. Am.-Eu.) **P. dubia* Hinde, 1879	–	–	–	–	–	–	–	X
P. sp. [X] Eichenberg, 1930	–	–	–	–	–	X	–	–
Scaliognathus Branson & Mehl, 1941 (**S. anchoralis* Branson & Mehl, 1942, L. Miss., N. Am.) ... *S.* sp. [Y] Branson & Mehl, 1941	–	–	–	–	–	[X]	–	–

† Not considered by Rhodes to be generically distinct as a discrete part from *Ligonodina* Ulrich & Bassler, 1926 (**L. pectinata*), Dev., N. Am.

‡ [X] An X in brackets denotes identifications made by an author subsequent to the one who named the assemblage.

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ANNEXE TO DOCUMENT 1/2

Dates of some genera of Conodonts

The type species of *Lonchodina*, *L. typicalis*, is sometimes credited to Bassler, 1925, sometimes to Ulrich & Bassler, 1926. In fact Bassler, 1925 : (1) lists genus *Lonchodina* ; (2) gives diagnosis of genus ; (3) lists : " Genotype, *L. typicalis*, new species ". In our view this leaves *L. typicalis* a *nomen nudum*, but some maintain generic diagnosis validates single species quoted. Ulrich & Bassler in 1926 refer additional spp. to *Lonchodina*, and generic diagnosis applies equally to all these species, as well as to the type species, here first described.

The same observations apply to *Synprioniodina* Bassler, 1925, and its type species *S. alternata* Ulrich & Bassler, 1926 ; to *Hindeodella* Bassler, 1925, and its type species, *H. subtilis* Ulrich & Bassler, 1926 ; *Prioniodella* Bassler, 1925, and its type species, *P. normalis* Ulrich & Bassler, 1926 ; and *Bryantodus* Bassler, 1925, and its type species, *B. typicus* Ulrich & Bassler, 1926.

DOCUMENT 1/3

Note on procedure : The paper reproduced below as Document 1/3 was originally submitted to the Office of the Commission at a time when (as explained in the Note on Procedure prefixed to Document 1/1 (page 5 above) the applicants contemplated the possibility of approval for their plan for the recognition of the category "para-taxon" being given by the International Commission by way of a *Declaration*. The paper given below was prepared as the second of a series of applications for the issue by the International Commission as soon as the "parataxa" scheme had been approved, of directions applying that scheme to particular groups. The decision to postpone the "parataxa" scheme until the London Congress in 1958 made it impossible to achieve any progress with the proposals submitted in the present paper. That paper is, however, placed on the London Agenda Paper for the purpose of illustrating the type of application which might be expected to be received by the Commission if the principal "parataxa" scheme were to be approved. Under the established procedure of the Commission proposals relating to individual names are not considered by it at meetings held during Congresses, it having been found more satisfactory that such cases should be studied in detail by the full Commission under the normal Three-Month Rule. Accordingly, it is not proposed that the detailed recommendations in regard to individual names contained in this paper shall be considered at the London Meeting. The papers will however be placed before the Commission for decision under the normal procedure as soon as possible after the close of that Meeting. [intl'd F.F. 11th July 1957.]

Second supplemental application : Application for a ruling by the International Commission directing that the classification and nomenclature of ammonoid aptychi (Class Cephalopoda) be in terms of "parataxa"

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(Commission Reference : Z.N.(S.) 1056)

The purpose of this application is to remove conflicts in nomenclature of mollusks belonging to the Subclass Ammonoidea of the Class Cephalopoda and to provide means for advancing continuity and universality of names used within this group. The problem encountered here consists of competition between independent sets of names which have come to be applied to different parts of these animals, known exclusively as fossils. These sets of names respectively comprise (1) generic and specific names based on the shell (or conch) which enclosed the soft parts of the living animal, and (2) generic and specific names based on the discrete internal (hyponomic) or opercular structures called aptychi, which after the death of the animal generally are not retained in the living chamber of the conch and therefore almost invariably are found separated from the conch. The first-mentioned group far outweighs the other in importance, because only the conch reveals morphological characters that can be used reliably for taxonomic classification ; also, it is probable that many ammonoids possessed no aptychi suitable for preservation as fossils. On the other hand, some known aptychi possibly belong to extinct cephalopods other than ammonoids, as for example belemnoids and soft-bodied dibranchiates. Whatever their origin, many aptychi are common enough and distinctive enough to have value in stratigraphic paleontology. Though they lack usefulness for distinguishing whole-animal taxa, there is need to describe them, illustrate them, and name them ; in fact, the nomenclature of aptychi already has developed to the extent of approximately 30 generic names and nearly 500 specific names. These aptychus names must be governed in manner to assure avoidance of conflict between them and names based on ammonoid conchs. The present application partly constitutes a counter proposal[1] to one submitted previously by W. J. Arkell (1954, *Bull. zool. Nomencl.* 9 : 266–269). We agree with him unreservedly in the wish to remove disturbance of nomenclature that arises from treating names for aptychi as correlative with names for ammonoid conchs but we are of the opinion that the suppression under Plenary Powers of all aptychus names would be undesirable in the extreme. The manner in which names published for aptychi introduces confusion in the nomenclature of ammonoids has been stated clearly by Arkell (*op. cit.*) and we urge that the problem presented should receive immediate attention of the Commission, for conflicts are even more numerous than might be inferred from the group of names cited by Arkell. The present application, which is supplementary to a preceding application of general scope on the subject of parataxa, seeks a Ruling of the Commission that the classification and nomenclature of fossils defined by the original author as aptychi should be in terms of parataxa. Reference to this application is being made in a volume of the *Treatise on Invertebrate Paleontology* containing systematic descriptions of Ammonoidea but in this volume the nomenclatural status of names published for aptychi necessarily must be left unsettled.

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[1] As will have been noted, the applicants in the present case state that their proposals represent, in part, a counter-proposal to an application previously submitted by Dr. W. J. Arkell. Accordingly, it has been decided to include Dr. Arkell's application among the documents submitted in connection with the plan for the recognition of the concept " parataxon " (i.e. to include that paper among the documents relating to Case No. 1 on the London Agenda Paper). Dr. Arkell's original application—on which no decision has as yet been taken by the International Commission—is accordingly being reprinted as Document 1/4 in the present volume (pp 71—75).

It will be understood that Dr. Arkell's proposals were directed solely to the removal, as he considered, of the threat to stability in the " normal " nomenclature of ammonites (i.e. in whole-animal nomenclature) represented by names based solely upon aptychi. It was in no way concerned therefore with the question dealt with in the present application by Professor Moore and Mr. Sylvester-Bradley for the recognition of " parataxa ", the proposals in regard to which had not at that time been submitted to the Commission. [intl'd F.H. 12th July 1957.]

2. Specialists in the study of ammonoid cephalopods are unanimous in concluding that the only reliable basis for discrimination of genera and species in this group of mollusks consists of morphological characters exhibited by the shell or conch. Accordingly, without exception, zoological names given to the ammonoid conchs have prevailed wherever a named conch has been found associated with a named aptychus in such a way as to allow no reasonable doubt that both belonged to the same animal. No one has yet been willing to accept the senior name of an aptychus as substitute for the name of a conch, despite stipulation of the *Règles* (Article 27) that the first-published name for a part of an animal shall be recognized for designation of the whole animal. A pertinent point which needs emphasis in this connection is the fact that prevailingly the characters displayed by aptychi are not diagnostic of genera and species based on conchs, and in consequence, some individual species of aptychi have been identified as belonging to two or more genera and species of ammonoids as defined by conchs. Therefore, in considering this problem it seems appropriate to accept as basic premises (1) the primary importance of nomenclature based on ammonoid conchs, and (2) the distinctly secondary importance of nomenclature based on aptychi. The present application is like that of Arkell in having the main purpose of establishing firmly names for ammonoids based on conchs by removing threats to their stability which come from aptychus names, but it differs in seeking to safeguard the usefulness of these latter. For the purpose of clear distinction generic and specific names based on ammonoid conchs, wherever cited in this application, are printed in boldface type, whereas all such names applied to aptychi are printed in italics, generic names recommended for acceptance being printed in capital letters. Designations employed simply as morphological terms (aptychus, anaptychus, synaptychus) are printed in roman letters without initial capitals. The type species of genera are marked uniformly by an accompanying asterisk (*).

3. Before attention is directed to the problems of nomenclature of aptychi the bearing of somewhat opposed stipulations of the revised *Règles*, both directed toward the stability and continuity of nomenclature, needs to be considered. One of these stipulations is observance of the Law of Priority, which in simplest terms means that the oldest valid name for a taxon shall prevail. On the other hand, the Principle of Conservation provides that the name of a generic or lower-rank taxon in general current use and available for 50 years or more shall not be replaced on nomenclatural grounds by a senior synonym unless the latter has been widely used during part of the 50-year period preceding proposed substitution. Provision has also been made for the suppression of *nomina dubia* and long-overlooked names by a challenge procedure that avoids use of the Plenary Powers (*Copenhagen Decisions zool. Nomencl.* : 24, 25). Several aptychus names are found to possess great antiquity of original publication, yet they have been long-overlooked and lack much, if any, subsequent use. Such names may appropriately be suppressed in favour of later-published, widely-used names and therefore recommendations to this effect are submitted in later paragraphs of the present application. We have

not, however, recommended adoption of the " challenge procedure " advocated at Copenhagen, for, as the Plenary Powers of the Commission need, in any case, to be invoked in other instances, it seems preferable to deal with the whole question of aptychi at one and the same time, rather than to adopt a procedure which would enforce a waiting period of two years before finalization of some but not all names concerned.

4. The following preliminary points are submitted :—

(1) It is the general purpose of this application to seek a Ruling that only those names which were defined by the original author as aptychi should be accepted as parataxa, special cases being dealt with through use of Plenary Powers. All names herein proposed for recognition fall into this category. It is a happy incident following this procedure that with a single exception (*SIDETES*) generic names of aptychi recommended for acceptance uniformly are compounds of various prefixes with the termination "-aptychus ". The first use of " aptychus " as a vernacular word which we have discovered is by Giebel in 1847 who referred to a special sort of undivided aptychus which he named *SIDETES.* He wrote : " . . . is ein besonders Aptychus von Salzburg sehr bemerkenswerth, denn seine Schalen berührten sich in der Mitte, Ich nenne ihn, da er generell eigentümlich ist, *Sidetes* ".

(2) We recommend that the following two specific names, which are designated as the type species of genera of aptychi, should be recognized by Plenary Powers as the names of parataxa, although introduced by their respective authors without statement that they represent aptychi :—

lamellosus Parkinson, 1811, as used in the combination *Trigonellites lamellosus* (specific name of the type species of *LAMELLAPTYCHUS* Trauth, 1927) ;

ceratoides Ooster, 1857, as used in the combination *Trigonellites ceratoides* (specific name of the type species of *LAEVILAMELLAPTYCHUS* Trauth, 1930).

(3) The generic name *Trigonellites* Parkinson, 1811, although not recognized by Parkinson as the name of an aptychus, is an objective senior synonym of the genus LAMELLAPTYCHUS Trauth, 1927. Both names are in current use, and it becomes necessary to decide between two alternative courses, both of which require use of the Plenary Powers. Either the name *Trigonellites* must be validated as the name of a parataxon (when it would, of course, take precedence

over *Lamellaptychus*), or it must be suppressed for the purposes of the Law of Priority to prevent conflict with conch names as listed below in para. 11 (1). Either course would stabilize nomenclature, but we believe that the latter would result in less preliminary name changing than the former, and have therefore recommended that the Plenary Powers should be used to suppress the name *Trigonellites* Parkinson, 1811.

5. Following is a chronologically arranged list of the generic names which have been applied to aptychi, those recommended for recognition as parataxa being printed in capital italic letters and those not thus recommended (including junior homonyms, junior objective synonyms, *nomina dubia*, and long-disused names which are recommended for suppression) being printed in small italic letters with initial capital. Synonyms are marked as objective by the abbreviation "obj." or as subjective by "subj.". Earliest authors variously misinterpreted the zoological affinities of aptychi, which came to be classed as lamellibranchs, fragments of fishes, or parts of crustaceans. Gradually, evidence has accumulated which serves to prove that the fossils called aptychi belong to cephalopods, many of them surely to ammonoids, but their taxonomic placement necessarily is subjective. Names based on species whose type-specimens were not recognized as aptychi by the original authors, but only subsequently so recognized, are preceded by a question mark. The morphological terms "aptychus", "anaptychus", and "synaptychus" (printed in small roman letters) are included in the list because each of these words has been published also as a generic name; explanations accompany the entries. The designation of type species of aptychus genera offers no problem if published names are based on aptychi, with type specimens consisting either of discrete aptychi or the aptychi of conch-and-aptychus associations, but difficulty is encountered where no separate and distinct specific name has been used for an aptychus. This matter is discussed in later paragraphs which deal with the conflict of aptychus and conch names.

? *Solenites* Gesner, 1758 (*Tract. phys. Petrificatis* : 39) [nec Mabille, 1887]. A *nomen dubium* possibly but not certainly applied to an aptychus, and in any case unavailable, as Gesner's *Tractatus* has been suppressed for nomenclatorial purposes by the International Commission under its Plenary Powers (*Opinion* 230).

? *Tellinites* Gesner, 1758(*op. cit.* : 38). Like *Solenites* Gesner, a *nomen dubium*, possibly but not certainly applied to an aptychus, but in any case not available, as Gesner's work has been suppressed (*Opinion* 230).

? *Trigonellites* Parkinson, 1811 (*Organic remains former world* 3 : 184). Type species (by subsequent selection, Moore and Sylvester-Bradley, 1956, *Treatise Invert. Paleont.*, *L*), **T. lamellosus* Parkinson, 1811 (: 186, pl. 13, figs. 10–11); =*Tellina cuneiformis* Baier, 1757 (*Monum. petrif. oryct. noricae*: 19, pl. 14, fig. 6–7), *non Tellina* Linne, 1758 (*T. radiata*, type species, a lamellibranch). [=*Solennites* Schlotheim, 1813 (subj.); *Aptychus* von Meyer, 1831 (subj.); *Muensteria* Eudes-Deslongchamps, 1835 (subj.); *Aptycus* Deshayes, 1845 (subj.); *Aptichus* Crosse, 1867 (subj.); *LAMELLAPTYCHUS* Trauth, 1927 (obj.); ? *PALAPTYCHUS* Trauth, 1927 (subj.)]. Recommended for suppression by the Plenary Powers. The junior objective synonym *LAMELLAPTYCHUS* is available as a replacement name for the taxon, and has been commonly so used in recent years.

? *Solennites* Schlotheim, 1813 (*Leonhard Taschenb.* 7(1) : 105). Type species (by monotypy), **S. annulatus* Schlotheim, 1813. A *nomen dubium*, possibly but not certainly applied to an aptychus. Recommended for suppression as a *nomen dubium* and as a long-overlooked name (lack of use subsequent to 1813).

? *Tellinites* Schlotheim, 1813 (*op. cit.*). Type species, none designated. A *nomen dubium*, possibly but not certainly applied to an aptychus, but certainly so used by Schlotheim, 1820 (*Petrefactenk*: 182, *Tellinites problematicus* Schloth., n. sp., fig'd by Baier, 1757, *Monum. petrif. oryct. noricae*: pl. 14, figs. 3, 8; =*Trigonellites latus* Parkinson, 1811; and *Tellinites solenoides* Schloth., n. sp., fig'd by Baier, 1757, pl. 14, figs. 6–7, =*Trigonellites lamellosus* Parkinson, 1811) and by Rüppel, 1829 (*Abbild. Verstein. Solenhofen*: 1–8), who redescribed and figured Schlotheim's species *T. problematicus* and *T. solenoides*. Recommended for suppression both as a *nomen dubium* and as a long-overlooked name (lack of use subsequent to 1829).

? *Ichthyosiagones* Bourdet de la Nièvre, 1822 (*Notice . . . fossiles inconnus*: 81). Type species (by monotypy, Rüppell, 1829, *Abbild. Verstein. Solenhofen*: 12, pl. 2, figs. 1–3), **Tellinites problematicus* Schlotheim, 1820 (=**Trigonellites latus* Parkinson, 1811, subj.). Senior subjective synonym of *LAEVAPTYCHUS* Trauth, 1927. Recommended for suppression as a long-overlooked name (lack of use subsequent to 1829).

Aptychus von Meyer, 1831 (*Nova Acta Leopold.-Carol. Akad. Naturf.* **15** (Abt. 2) : 125). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956, *op. cit.*), **A. imbricatus* von Meyer, 1831 (: 127, pl. 59, figs. 1–12) (=**Trigonellites lamellosus* Parkinson, 1811, *fide* von Meyer, 127; *fide* Giebel, 1851, *Fauna Vorwelt* **3** : 768; *fide* Trauth, 1938, *Palaeontograph.* **88A** : 149; confirmed Moore & Sylvester-Bradley, 1956) [=*Trigonellites* Parkinson, 1811 (subj.); *Muensteria* Eudes-Deslongchamps, 1835 (subj.); *Aptycus* Deshayes, 1845 (obj.); *Aptichus* Crosse, 1867 (obj.); *LAMELLAPTYCHUS* Trauth, 1927 (subj.); *Palaptychus* Trauth, 1927 (subj.)]. The date of von Meyer's publication has been cited erroneously by many authors as 1829 (when the paper was read but not when it was published); it is correctly cited by Neave (*Nomencl. zool.* **1** : 268) as 1831. Unquestionably, the designation *Aptychus* was originally published as a generic name and it has been used in this way by many authors in the period 1837–90, by a few in the period 1890–1927 (to the time of beginning of Trauth's extensive studies), and virtually not at all since 1927. Meanwhile, aptychus (pl. aptychi), used as a common noun, has gained universal currency as a morphological term, which is employed to designate the presumed opercular structure of many ammonoids and possibly of some other cephalopods. *Aptychus* is recommended for suppression as a generic name on the grounds that (1) it has disappeared from common use as such in paleontological literature; (2) it has come to be replaced by other generic names (both *sensu lato* and *sensu stricto*); (3) it induces confusion with aptychus and aptychi as morphological terms; and (4) it presents an analogy between the proposed suppression of the name *Aptychus* and the suppression of the name **Ammonites** under the Plenary Powers (*Opinion* 305) but in this case *Aptychus* cannot be claimed a *nomen dubium.* The case for the suppression of the name *Aptychus* rests, therefore, mainly on the fact that its reintroduction into the literature as a generic name would cause it to preoccupy, as a senior subjective synonym, the currently used name *LAMELLAPTYCHUS*, whereas its suppression would cause no name changes.

? *Muensteria* Eudes-Deslongchamps, 1835 (as *Münsteria*) (*Mém. Soc. linn. Normandie* 5 : 61) [*non* Knebel, 1909 (*Arch. Biont.* 2 : 222)]. Type species by subsequent selection, Moore & Sylvester-Bradley, 1956, *op. cit.*), **M. sulcata* Eudes-Deslongchamps, 1835 (: 66, pl. 2, figs. 10–11) (=**Trigonellites lamellosus* Parkinson, 1811). [=*Trigonellites* Parkinson, 1811 (subj.); *Aptychus* von Meyer, 1831 (subj.); *LAMELLAPTYCHUS* Trauth, 1927 (subj.)]. Recommended for suppression as a long-overlooked name (lack of use since 1835).

Aptycus Deshayes, 1845 (*in* Lamarck, *Hist. Anim. s. Vert.* ed. 2, **11** : 228) [*pro Aptychus* von Meyer, 1831 (obj.)]. Recommended for suppression as an invalid subsequent spelling of *Aptychus.*

SIDETES Giebel, 1847 (*N. Jahrb. Mineral.* **1847** : 821). Type species (by monotypy, Giebel, 1849, *Zeitschr. Deutsch. Geol. Gesell.* **1** : 99, text-fig. p. 100), **S. striatus* Giebel, 1849. [=*Anaptychus* Schlumberger, 1868 (subj.) (*non* Stimpson, 1860); *Pholidocaris* H. Woodward, 1882 (subj.); *Cardiocaris* H. Woodward, 1882 (subj.); ? *Ellipsocaris* H. Woodward, 1882 (subj.); ? *Lisgocaris* Clarke, 1882 (subj.); *Palanaptychus* Trauth, 1927 (subj.); ? *NEOANAPTYCHUS* Nagao, 1931 (subj.)]. The type species and its type specimen are based on an isolated aptychus found in Upper Cretaceous strata and association of this type species with ammonoid conchs is unknown. On the other hand, several other species of *SIDETES* are represented by specimens forming parts of conch-and-aptychus associations. As the name *SIDETES* was stated by Giebel to be based on a special kind of aptychus, it can be classed as a parataxon by operation of the general Ruling recommended for adoption in this application. It is currently used as a generic name for aptychi.

? *Ichthyosiagon* Herrmannsen, 1847 (*Index Gen. Malac.* **1** : 555) [*pro Ichthyosiagones* Bourdet de la Nievre, 1822 (obj.)]. Recommended for suppression as an invalid subsequent spelling of *Ichthyosiagones*.

aptychus (pl. aptychi) Giebel, 1847 (*N. Jahrb. Mineral.* **1847** : 821), Oppel, 1856 (*Jahresh. Ver. vaterl. Naturk. Württemberg* **12** : 107, 194), common noun used solely as a morphological term and subsequently almost universally employed in this way by authors. No consideration or action by ICZN in connection with a morphological term is necessary or appropriate.

anaptychus (pl. anaptychi) Oppel, 1856 (*op. cit.* : 194) as indicated by context, a common noun introduced and used solely as a morphological term for an undivided type of aptychus and subsequently so used almost universally by authors. As stated in discussion of *Anaptychus* Schlumberger, 1868, the citation of Oppel as author of *Anaptychus* as a generic name is, in our view, erroneous. The Commission is not concerned with anaptychus as a morphological term. However, the name *Anaptychus* is listed by Neave (*Nomenclator Zoologicus*) as a genus dating from Oppel, and has often thus been regarded. We recommend that the Commission should remove all ambiguity by directing that the name " anaptychus " as used by Oppel, 1856, is not to be regarded as the name of a genus, but only as a morphological term. [Oppel (1856 : 194) first refers to this aptychus in general terms, thus "*Ammonites planorbis* zeichnet sich durch den Besitz eines ungespaltenen Aptychus* aus," ; in a footnote he introduces the new name : *(Footnote) Veilleicht besser "Anaptychus" (αναπτυχος entfaltet), da dieser innere Theil des *Ammonites planorbis* von Aptychus anderer Ammoniten gänzlich verschieden ist."].

Aptichus Crosse, 1867 (*J. Conchyliol.* **15** : 456) [*pro Aptychus* von Meyer, 1831 (obj.)]. Recommended for suppression as an invalid subsequent spelling of *Aptychus*.

Anaptychus Schlumberger, 1868 (*Bull. Soc. linn. Normandie* (2) **1** : 97) [*non* Stimpson, 1860, *Ann. Lyceum nat. Hist. N.Y.* **7** : 183 (Crust.)]. Based on three conch-and-aptychus associations, two identified without query with the following conch species : *Ammonites laqueus* and *Amaltheus margaritatus*. If we are right in our opinion that Oppel, 1856, introduced anaptychus as a morphological term only, the nomenclatural status of *Anaptychus* as a generic name has nothing to do with Oppel. In 1860 Stimpson published this name for a group of crustaceans, making Schlumberger's use of the same name for designation of ammonoid aptychi a junior homonym and therefore invalid. Accordingly, *Anaptychus* Schlumberger, 1868, is recommended for suppression as a junior homonym. In 1928 Strand (*Arch. Naturgesch.* **92** (A8) : 40), thinking that *Anaptychus* Stimpson, 1860, had been pre-occupied by Oppel, 1856, introduced the new name *Anaptychoides* to replace Stimpson's *Anaptychus*, which was actually valid. *Anaptychoides* Strand, 1928, is therefore a junior objective synonym of the crustacean *Anaptychus* Stimpson, 1860, but whether either name has actually been used is a question which should be referred to specialists on the Crustacea.

? *Pholadocaris* H. Woodward, 1882 (*Geol. Mag.* (2) **9** : 338). Type species (by monotypy), **P. leeii* Woodw., 1882 (: 388, pl. 9, fig. 16). [=*SIDETES* Giebel, 1847 (subj. ; *fide* Trauth, 1935, *Jahrb. Preuss. Geol. Landesanst.* **55** : 69 ; *fide* Moore & Sylvester-Bradley, 1956)]. Because the type species has been only subjectively identified with an aptychus subsequently to original publication, it is not recommended that the name should be added to the *Official Lists* as that of a parataxon.

? *Cardiocaris* H. Woodward, 1882 (*Geol. Mag.* (2) **9** : 386). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **C. roemeri* Woodw., 1882 (: 386, pl. 9, figs. 1–3). [=*SIDETES* Giebel, 1847 (subj. ; *fide* Trauth, 1935, *op. cit.* : 65 ; *fide* Moore & Sylvester-Bradley, 1956)]. This name, like that of *Pholadocaris* is based on a type-species only subsequently identified with an aptychus. It is not recommended that it should be added to the *Official Lists* as a parataxon.

Ellipsocaris H. Woodward, 1882 (*Geol. Mag.* (2) **9** : 444). Type species (by monotypy), **E. duwalquei* Woodw., 1882 (: 445, fig. 4). (?=*SIDETES* Giebel, 1847 (subj. ; *fide* Trauth, 1935, *op. cit.* : 58 ; *fide* Moore & Sylvester-Bradley, 1956)]. It is not recommended that this name, with type species only subsequently identified with an aptychus, should be added to the *Official Lists* as that of a parataxon.

? *Lisgocaris* Clarke, 1882 (*Am. J. Sci.* (3) **23** : 478). Type species (by monotypy), **L. lutheri* Clarke, 1882 (: 478, fig. 5). [?=*SIDETES* Giebel, 1847 (subj. ; *fide* Trauth, 1935, *op. cit.* : 67 ; *fide* Moore & Sylvester-Bradley, 1956)]. It is not recommended that this name, with type species only subsequently identified with an aptychus, should be added to the *Official Lists* as that of a parataxon.

synaptychus (pl. synaptychi) Fischer, 1882 (*Manuel Conchyliol. Paléont.* : 377), common noun apparently intended as a morphological term rather than a genus (for example, Fischer writes under the description of *Scaphites spiniger* "Aptychus à surface granuleuse ou striée ressemblant à celui des *Perisphinctes*. Les deux pìeces se soudent sur la ligne mediane chez les *S. spiniger* et *Römeri* (Schlüter). Cette forme d'*Aptychus* peut être appelée *Synaptychus* ". Nowhere does he use *Synaptychus* in combination with a specific name so as to form a binomen). As stated in discussion of *Synaptychus* Basse, 1952, the citation of Fischer as author of *Synaptychus* as a generic name is therefore, in our view, erroneous. The Commission is not concerned with synaptychus as a morphological term. However, as in the case of anaptychus, this name has been listed by Neave (*Nomen-*

clator Zoologicus) as a genus dating from Fischer, 1882, and we recommend that the Commission should, in order to remove all ambiguity, direct that the name " synaptychus " as used by Fischer, 1882, is not to be regarded as the name of a genus, but only as a morphological term.

CORNAPTYCHUS Trauth, 1927 (*Ann. naturhist. Mus. Wien* **41** : 189, 228). Type species (by monotypy), **Aptychus hectici* Quenstedt, 1849 (*Petrefactenk. Deutschl.* **1** : 119, 315, pl. 8, fig. 10). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

CRASSAPTYCHUS Trauth, 1927 (*op. cit.* : 205, 228). Type species (by monotypy), **Aptychus crassus* Hébert, 1885 (*Mem. Soc. Geol. France* **5** : 368, pl. 28, figs. 8a, b). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

GRANULAPTYCHUS Trauth, 1927 (*op. cit.* : 217, 228). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **G. suevicus* Trauth, 1930 (*Ann. naturhist. Mus. Wien* **44** : 389, pl. 5, figs. 3–5 ; lectotype defined by Moore & Sylvester-Bradley, 1956, as original specimen represented by Trauth's fig. 4) ; no species were assigned to the genus in 1927 but descriptions and illustrations of 4 species were published by Trauth in 1930, *G. suevicus* being one of these. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

LAEVAPTYCHUS Trauth, 1927 (*op. cit.* : 189, 228). Type species (by monotypy), **Aptychus meneghinii* Zigno, 1870 (*Mem. reale Ist. Venete Sci. Arte* **15** : 11, pl. 8, figs. 1–4)[=*Ichthyosiagones* Bourdet, 1822 (subj.), a name proposed for suppression as long-overlooked]. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

LAMELLAPTYCHUS Trauth, 1927 (*op. cit.* : 189, 228). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Trigonellites lamellosus* Parkinson, 1811 (*op. cit.* : 184, pl. 13, figs. 10–11). [=*Trigonellites* Parkinson, 1811 (obj.), a name proposed for suppression under the Plenary Powers]. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

LISSAPTYCHUS Trauth (*op. cit.* 220, 231). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Aptychus leptophyllus* Sharpe, 1856 (*Mon. Palaeontograph. Soc.* : 55, pl. 24, figs. 1a, b]. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

? *Palanaptychus* Trauth, 1927 (*op. cit.* : 203, 214). Type species (by monotypy), ***Manticoceras intumescens*** (von Bayrich). [=*SIDETES* Giebel, 1847 (subj.)]. Recommended for suppression by the Plenary Powers on the grounds of nomenclatural ambiguity (since the type-specimen of the type-species is a conch and not the conch-and-aptychus association on which the name *Palanaptychus* is based). The name, moreover, has not been used since its original introduction, and cannot be used for an aptychus as a parataxon if its type-species has a conch for a type-specimen. Nor can the name be used for a conch genus without such violation of the author's original intent as to introduce grave ambiguity. The case is further discussed in para. 9(2) below.

PALAPTYCHUS Trauth, 1927 (*op. cit.* : 214). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Aptychus carbonarius* Koenen, 1879 (*N. Jahrb. Mineral.* **1879** : 317), one of the two originally included species. [?=*LAMELLAPTYCHUS* Trauth, 1927 (subj.)]. Although doubtfully distinguished from *LAMELLAPTYCHUS*, we recommend that it should be entered as a parataxon on the *Official List* for the use of any taxonomist who believes it to represent a parataxon distinct from that genus.

PRAESTRIAPTYCHUS Trauth, 1927 (*op. cit.* : 219, 230). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **P. gerzensis* Trauth, 1930 (*op. cit.* : 380, pl. 5, figs. 14–15 ; lectotype defined by Moore & Sylvester-Bradley as the specimen represented by Trauth's fig. 14). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

PSEUDOSTRIAPTYCHUS Trauth, 1927 (*op. cit.* : 220, 231). Type species (by monotypy, Trauth, 1928, *Ann. naturhist. Mus. Wien* **42** : 168), **P. pseudostobaei* Trauth, 1928. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

PTERAPTYCHUS Trauth, 1927 (*op. cit.* : 188, 218). Type species (by monotypy), **Aptychus numida* Coquand, 1854 (*Mem. Soc. géol. France* (2) **5** : 140, 148, pl. 3, fig. 1). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

PUNCTAPTYCHUS Trauth, 1927 (*op. cit.* : 200, 228). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Aptychus punctatus* Voltz, 1837 (*N. Jahrb. Mineral.* **1837** : 435 ; Trauth, 1935, *Jahrb. Geol. Bundesanst. Wien* **75** : 315, pl. 12, figs. 1–6), chosen from 3 nominal species cited by Trauth in his 1927 publication as belonging to the genus, although one of these (*Aptychus profundus* Pictet) was synonymized with *A. punctatus* by Trauth in 1935 (: 315). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

RUGAPTYCHUS Trauth, 1927 (*op. cit.* : 220, 228). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Aptychus rugosus* Sharpe, 1856 (*op. cit.* : 57, pl. 24, figs. 8–9 ; Trauth, 1928, *op. cit.* : 122, pl. 2, figs. 1–4), chosen from 4 species referred to genus in Trauth's original publication. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

SPINAPTYCHUS Trauth, 1927 (*op. cit.* : 200, 220). Type species (by monotypy), **Aptychus spinosus* Cox, 1926 (*Ann. Mag. nat. Hist.* (9) **17**: 577, pl. 24, figs. 1–3 ; Trauth, 1928, *op. cit.* : 131, pl. 3, figs. 17–18). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

STRIAPTYCHUS Trauth, 1927 (*op. cit.* : 189, 229). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **S. spinigeri* Trauth, 1927 (244), chosen from 20 species and subspecies listed in Trauth's original paper as belonging to this genus. [=*Synaptychus* Basse, 1952 (obj.)]. For further discussion of synonymy see below under *Synaptychus* Basse, 1952 and para. 9(1). Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

LAEVILAMELLAPTYCHUS Trauth, 1930 (*op. cit.* : 336). Type species (by subsequent selection, Moore & Sylvester-Bradley, 1956), **Trigonellites ceratoides* Ooster, 1857 (*Petrif. remarquables Alpes Suisses* (2) : 16, pl. 6, figs. 6–8 ; Trauth, 1930, *op. cit.* : 366, pl. 4, figs. 9–10), chosen from 3 species referred without question to the genus in Trauth's 1930 publication. Recommended for entering on appropriate *Official List* as a parataxon available for designation of aptychi but not ammonoid conchs or conch-and-aptychus associations.

NEOANAPTYCHUS Nagao, 1931 (*Proc. Imp. Acad. Japan* **7** : 109). Type species (by monotypy), **N. tenuiliratus* Nagao, 1931 (: 109, figs. 1–2). Trauth in 1935 (*N. Jahrb. Mineral.* **73B** : 75 ; *ibid.* **74B** : 459) invalidly designated *N. semicostatus* Nagao, 1932 (*Proc. Imp. Acad. Japan* **8** : 175, text-fig. p. 178) as the type species. [=?*SIDETES* Giebel, 1847 (subj.)]. Recommended for entering on appropriate *Official List* as a parataxon for use of any taxonomist who believes it to represent a parataxon distinct from *SIDETES*.

Laevicornaptychus Trauth, 1936 (*Ann. naturhist. Mus. Wien* **47** : 28). Not an available name, as no type species was indicated subsequent to 1930.

Synaptychus Basse, 1952 (*Traité Paléont.* 2 : 548). Type species (by monotypy), **STRIAPTYCHUS spinigeri* Trauth, 1927 (erroneously cited by Basse as "*Synaptychus spiniger* Trauth"). Schlüter, 1872 (*Palaeontographica* **21** : 82–85, pl. 25, figs. 1–7) in describing a new species of ammonoid named by him, **Scaphites spiniger,** mentioned two specimens which contain "den zugehörigen Aptychus" in the living chamber, one of these being illustrated (pl. 25, figs. 5–7) ; this figured specimen with its bipartite aptychus in position has been designated (Moore & Sylvester-Bradley, 1956) as lectotype of Schlüter's species, now known as **Acanthoscaphites spiniger.** It is the aptychus of this specimen that was refigured by Fischer, 1882 (*op. cit.* : 377) with the designation "synaptychus of **Scaphites spiniger**" and accompanying discussion which makes clear Fischer's use of synaptychus only as a morphological term. Trauth, 1927 (*op. cit.* : 244) introduced for this aptychus (or synaptychus) the name "*STRIAPTYCHUS spinigeri* Trauth, n. n.". Basse, 1952 (: 549, figs. 12, 19) copied Schlüter's pl. 25, fig. 6, incorrectly attributing it to Reeside, and (: 549, fig. 12, 12) reproduced Schlüter's pl. 25, fig. 7, with identification of it as "*Synaptychus spiniger* (Trauth)". As matters stand, Basse is the first author to publish *Synaptychus* as a generic name and the holotype of its type species is part of the same specimen that serves **Acanthoscaphites spiniger** (Schlüter), as holotype *Synaptychus* Basse, 1952, is recommended for suppression as a junior objective synonym of *STRIAPTYCHUS* Trauth, 1927.

6. A large majority of the binomina which have been published as scientific designations for different aptychi are composed of generic and specific names that are quite independent of those used for ammonoid conchs. For example, the name *SIDETES striatus* Giebel, 1849, was given to an undivided type of aptychus found in Upper Cretaceous strata, the type specimen being an isolated aptychus, and neither the name *SIDETES striatus* nor fossils belonging to this species are related to a known conch. Such aptychi may have value in stratigraphic paleontology and are not involved in nomenclatural conflicts until representatives of a given species happen to be found in association with a named species of conch.

7. Specimens of aptychi sometimes are found in the living chambers of ammonoid conchs or neatly filling the shell apertures in the manner of opercula ; such associations leave no reasonable doubt that the aptychus and conch were produced by a single animal. Under operation of the present *Règles*, every occurrence together of a genus and species of aptychus with a genus and species of conch, each independently named, introduces questions of synonymy which are illustrated and discussed in following paragraphs. At least 40 genera defined by characters of the conchs are involved in nomenclatoral conflicts of this nature. We believe that none of these genera or others which may be found associated with aptychi should be synonymized with aptychus names and hence propose in para. 14 of this application that nominal genera and species of aptychi shall be defined as parataxa, thus remaining subject to the *Règles* but being denied status which permits interference by them with names published for conchs. As applied to aptychi occurring in association with ammonoid conchs in manner that clearly indicates their belonging together in life, three modes of nomenclatural treatment of the aptychi are found in the literature. Each of them presents problems and is unsatisfactory from the standpoint of trying to apply the present *Règles*. This is demonstrated by brief explanation and illustration.

(1) A common method of referring to the aptychi of conch-and-aptychus associations, especially in older publications, has been to use only the morphological term " aptychus " in combination with the name of the conch, as " aptychus of **Baculites knorrianus** " (Schlüter, 1876, p. 27), without other designation. This conforms to the *Règles* and is entirely adequate for description of the conch-and-aptychus specimen but isolated aptychi of seemingly identical sort cannot safely be identified as belonging to this conch species. It would be misleading to describe beds with many such aptychi but lacking specimens of conchs as " characterized by abundant **Baculites knorrianus** ".

(2) A second method of designating the aptychi of conch-and-aptychus associations is to use the specific name of the conch combined with a generic name different from that of the conch and denoting a particular sort of aptychus, as in the following example :

(a) The aptychus mentioned in subparagraph (1) has been cited (Diener, 1925, *Fossilium Catalogus*, **29** : 40) as "*Aptychus knorrianus* Schlüter ", which combines the generic name *Aptychus* von Meyer, 1831, with the specific name **knorrianus**, originally published by Demarest, 1817, in combination with the conch name *Baculites*. The combination "*Aptychus knorrianus* " was never used by Schlüter, who merely referred to the " aptychus of **Baculites knorrianus** ". The attribution to Schlüter is therefore clearly false.

(b) In 1927, Trauth (*op. cit.* : 245) published the name " *Rugaptychus knorrianus* (Schlüter) " for this fossil, changing the generic name and repeating Diener's error in attributing authorship of the species to Schlüter.

(c) Ambiguity persists because it is not clear whether the specific name *knorrianus* as used in the combination *Aptychus knorrianus* should be interpreted as a new name introduced by Diener [leading to designation as "*RUGAPTYCHUS knorrianus* (Diener)", with type specimen the aptychus figured in association with a conch by Schlüter], or referred to Desmarest [leading to "*RUGAPTYCHUS* **knorrianus** (Desmarest)", with type specimen the conch (without atypchus) figured by Desmarest in 1817]. With recognition of *RUGAPTYCHUS* as a parataxon, the former alternative becomes the only possible one, for *RUGAPTYCHUS* can only be a parataxon if its type species is based on an aptychus.

(d) A number of the genera which we propose should be recognized as parataxa have type species cited in this ambiguous manner. To remove any doubt as to what specimens should be regarded as type specimens of these species, we recommend that the Plenary Powers should be used to specify the holotype in the following cases:

GRANULAPTYCHUS suevicus Trauth, 1930, to be interpreted by reference to the specimen figured under this name by Trauth (1930 : pl. 5, fig. 4);

PSEUDOSTRIAPTYCHUS pseudostobaei Trauth, 1928, to be interpreted by reference to the unfigured specimen described by Schlüter, 1872 (58) and Trauth, 1928 (168) from Coesfelder Berge, Germany;

NEOANAPTYCHUS tenuiliratus Nagao, 1931, to be interpreted by reference to the specimen figured under this name by Nagao, 1931 (109, fig. 1).

(3) A third method of referring to the aptychi of conch-and-aptychus associations is to employ a name for the aptychus that is wholly independent of the conch name (as noted in para. 6). This avoids confusion of the sort considered in the preceding subparagraph, and is the most common method in current use. However, it is incompatible with present *Régles* which allow only a single name for the conch-and-aptychus, for the conch alone, or for the aptychus alone. With the recognition of parataxa, however, it would be the recommended method. Thus *Anaptychus pala* Trauth, 1935 (*N. Jahrb. Mineral.*, **73B** : 85) is a name given to the aptychus of **Amaltheus margaritatus** de Montfort, 1808; *LAEVAPTYCHUS latus* (Parkinson), 1811, is the name given to the aptychus of **Euaspidoceras perarmatum** (Sowerby), 1822; *SIDETES sellaeformis bicarinatus* (Trauth), 1935, is designation of the aptychus of **Asteroceras stellare** (Sowerby), 1815; and *LAMELLAPTYCHUS beyrichi* (Oppel), 1865, is the name of an aptychus belonging to **Haploceras elimatum** (Oppel), 1868. All of these examples introduce problems of synonymy as

interpreted by the present *Règles* which are discussed in later paragraphs. These problems vanish with the recognition of parataxa.

8. As previously stated, every association of conch-and-aptychus which leads one to believe that the two were derived from a single animal, results, on applying Article 27 of the present *Règles*, to a conflict between the names applied respectively to the conch and aptychus. The problem of synonymy is complicated by the fact that the taxonomic bounds of many aptychus species are currently drawn very much more widely than those of any conch species, as is indicated by published records of a single aptychus species classed as belonging to two or more different species (or even different genera) of conchs. Therefore, it is not possible to determine what species of conch is represented by an isolated specimen of aptychus, even if other specimens, subjectively identified with the isolated one, are found in association with a conch species. For example, Trauth, 1938 (130) identified the aptychus species *LAMELLAPTYCHUS rectecostatus* (Peters), 1854, as belonging to four different conch species classed in three different genera—**Oppelia bous** (Oppel), **O. euglypta** (Oppel), **Neochetoceras steraspis** (Oppel)*, and **Haploceras lingulatum solenoides** (Quenstedt). Under such circumstances, synonymization according to the present *Règles* is impossible at the specific level and the whole situation is nomenclaturally unstable. If the names based on aptychi are classed as parataxa, conflicts of this sort will disappear. The extreme complication of the synonymy involved, and the undesirable nomenclatural situation that would arise if the present *Règles* were applied, is illustrated in the following paragraphs 9–13. The degree of subjectivity concerned is first determined by the firmness of identifying the species involved in the questions of synonymy. Among all recorded ammonoid conch-and-aptychus associations none constitutes the type specimen of the type species both of a nominal genus of conchs and nominal genus of aptychi, thus providing for objective identification of one with the other. Indeed, no conch genus with type species defined by a type specimen consisting of a conch-and-aptychus association is known. There are some cases, however, in which the type species of an aptychus genus has as type specimen conch or conch-and-aptychus association, which is also type specimen of a conch species. These cases therefore involve objective synonymy between the aptychus species and the conch species and are dealt with in paragraph 9. All other cases of synonymy between conch and aptychus are subjective in that they involve type specimens which are not identical. Four different classes of subjective synonyms can be distinguished as follows :—

(1) synonymy involving subjective identification of the type species of an aptychus genus with the type species of a conch genus (see paragraph 10);

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* At the same time Trauth identified *Lamellaptychus theodosia longa* Trauth as belonging also to **Neochetoceras steraspis.** This must surely be a case of misidentification.

(2) synonymy involving subjective identification of the type species of an aptychus genus with a conch species other than the type species of the genus to which it is assigned (see paragraph 11) ;

(3) synonymy involving subjective identification of the type species of a conch genus with a species of aptychus other than the type species of the genus to which it is assigned (see paragraph 12) ;

(4) synonymy involving subjective identification of a conch species and an aptychus species, neither of which is the type species of a genus (see paragraph 13).

9. This paragraph deals with those cases in which type species of aptychus genera are, according to the present operation of the *Règles*, objectively synonymous with species of conch genera, being based on the same type specimens. Five aptychus genera are involved. The fact that three of these (*Anaptychus*, *Synaptychus*, *Palanaptychus*) are respectively classed as a junior homonym, junior objective synonym, and junior subjective synonym, the other two (*STRIAPTYCHUS*, *PSEUDOSTRIAPTYCHUS*) being considered valid, is incidental to the nomenclatural problems presented by their conch associations. These problems are discussed briefly.

(1) *STRIAPTYCHUS* and *Synaptychus* versus **Acanthoscaphites.** *Synaptychus* Basse, 1952, is a junior objective synonym of *STRIAPTYCHUS* Trauth, 1927, since the type specimen of the type species of both is the same, consisting of the aptychus of a conch-and-aptychus association which is also the type specimen of **Acanthoscaphites spiniger** (Schlüter), 1872. The name of the aptychus species is *STRIAPTYCHUS spinigeri* Trauth, 1927. The nominal genus **Acanthoscaphites** was introduced by Nowak in 1911 (type species, ***Scaphites tridens** Kner, 1848). This is a clear-cut case which according to the *Règles* leaves no possibility of doubt in concluding that *STRIAPTYCHUS spinigeri* is a junior objective synonym of **Acanthoscaphites spiniger** and that *STRIAPTYCHUS* is a junior subjective synonym of **Acanthoscaphites,** even though some aptychi which seem surely classifiable as *STRIAPTYCHUS* are known to belong to other conch genera. For example, *S. cheyannensis* occurs in the living chamber of a conch identified as **Discoscaphites cheyennensis** (Meek). With recognition of *STRIAPTYCHUS spinigeri* as a parataxon (as we recommend) the name *STRIAPTYCHUS* becomes a permissible generic name for such aptychi ; otherwise the name must either be suppressed, as a synonym of **Acanthoscaphites**, or become designation of a new genus of conchs with **Acanthoscaphites spiniger** as type species.

(2) *Palanaptychus* versus **Manticoceras.** The name *Palanaptychus* was published by Trauth 1927 (*op. cit.* : 234) to designate the undivided aptychus in the conch of a specimen of **Manticoceras intumescens** (von Beyrich) from Devonian rocks. This species becomes type of *Palanaptychus* by monotypy. As such, however, the species is based not on the conch-and-aptychus association on which the name *Palanaptychus* was founded, but on the conch (without aptychus) which forms the type specimen of **M. intumescens.** According to the *Règles* **Manticoceras** (type species, ***Goniatites simulator** Hall, 1874) is therefore a senior subjective synonym of *Palanaptychus*, but if the type specimen of the latter should be interpreted as generically distinct from **Manticoceras** and from **Gephuroceras** Hyatt, 1884 (=**Gephyroceras** Frech, 1901, obj.) and **Crickites** Wedekind, 1913, which now are classed as subjective synonyms of **Manticoceras,** *Palanaptychus* would be an available name. This application proposes the suppression of *Palanaptychus*, however, as we believe that to use the name for a genus of conchs based on **M. intumescens** would be most objectionable.

(3) *PSEUDOSTRIAPTYCHUS* versus **Parapachydiscus.** The genus *PSEUDOSTRIAPTYCHUS* was introduced by Trauth, 1927, for an aptychus found in association with a conch identified as **Parapachydiscus pseudostobaei** (Moberg, 1885). No other specimen was mentioned. This species therefore becomes type of *PSEUDOSTRIAPTYCHUS* by monotypy, and, as in the case of *Palanaptychus*, we are left in the unsatisfactory situation in which the type species of an aptychus genus has a type specimen which is a conch without aptychus. **Parapachydiscus** Hyatt, 1900 (type species, ***Ammonites gollevillensis** Sharpe, 1857) is a junior subjective synonym of **Pachydiscus** Zittel, 1884 (type species, ***Ammonites neubergicus** Hauer, 1858, by subsequent designation, de Grossouvre, 1894). However, if **Pachydiscus pseudostobaei** (Moberg) were deemed to be generically distinct from both **Pachydiscus** and **Para pachydiscus,** then *PSEUDOSTRIAPTYCHUS* would be an available name for such a conch genus. This we believe would be as objectionable as to use *Palanaptychus* as the name of a genus of conchs. If, however, *PSEUDOSTRIAPTYCHUS* is ruled to be a parataxon, as we recommend, it is necessary to stipulate that its type species must be based on an aptychus. In 1928 Trauth (*op. cit.* : 168) named the aptychus associated with Moberg's species as " *PSEUDOSTRIAPTYCHUS pseudostobaei* Trauth, n.n." Inasmuch as this name was applied to a specimen of aptychus identified with **Parapachydiscus pseudostobaei,** it is based on a type specimen different from that of the species of conch described by Moberg and therefore is a new species as well as a new name. Moreover, since this is the only species then referred without question to the genus, it may be recognized as the type species by subsequent monotypy. In order

to regularize the choice of type species without ambiguity, we recommend that the Commission use its Plenary Powers to validate *PSEUDOSTRIAPTYCHUS pseudostobaei* Trauth, 1928, as the type species.

(4) *Anaptychus*. Oppel used the word " anaptychus " as a morphological term in 1856. It was not until 1868 that Schlumberger introduced *Anaptychus* as a generic name, including within it those conch-and-aptychus associations identified with *Amaltheus margaritatus* de Montfort and *Ammonites laqueus* Quenstedt. The name is, however, a junior homonym, as it had already been used in 1860 by Stimpson for a genus of Crustacea. It must therefore be suppressed, and it becomes unnecessary to choose a lectotype. If, however, it had been a valid name, it would have raised the same problems as *PSEUDOSTRIAPTYCHUS.*

10. This paragraph deals with synonymy involving subjective identification of the type species of an aptychus genus with the type species of a conch genus. Nomenclatural conflicts which are encountered in applying the present *Règles* can in each case be avoided by defining the aptychus names as parataxa. The type species of both conch genera and aptychus genera are indicated by an asterisk, and if synonymization involves the type specimen of an aptychus species, this is indicated by " holotype " or " lectotype " in parentheses following the specific name, but otherwise identifications are based on specimens other than type specimens.

(1) *PUNCTAPTYCHUS* versus **Haploceras.** The aptychus genus *PUNCTAPTYCHUS* Trauth, 1927, is based on the type species **Aptychus punctatus* Voltz, 1837. The conch genus **Haploceras** Zittel, 1870, has the type species ***Ammonites elimatus** Oppel in Zittel, 1868 (by subsequent selection, Spath, 1923). Trauth, 1935 (*Jahrb. Geol. Bundesanst. Wien* **85** : 314) records the occurrence together of these two type species in a conch-and-aptychus association which is not the type specimen of either species. If the identification of these components is accepted, the *Règles* would require rejection of *PUNCTAPTYCHUS* as a junior subjective synonym of **Haploceras** and the specific name **elimatus** would be displaced similarly by *punctatus*, introducing great nomenclatural confusion both as regards designation of other species now assigned to *PUNCTAPTYCHUS* and coordination with the problem of **Haploceras** versus *LAMELLAPTYCHUS* given in para. 12, subpara. 10 below. Such confusion is avoided if the aptychus names are defined as parataxa.

(2) *CORNAPTYCHUS* versus **Hecticoceras.** The aptychus genus *CORNAPTYCHUS* Trauth, 1927, is based on **Aptychus hectici*

Quenstedt, 1849 (monotypy). The conch genus **Hecticoceras** Bonarelli, 1893, is based on the type species ***Nautilus hecticus** Reinecke, 1818 (monotypy). The holotype of *C. hectici* occurs in the living chamber of a conch identified by Quenstedt as **Ammonites hecticus** (Reinecke) (Trauth, 1930, *op. cit.* : 351). According to the *Règles*, the valid name of both conch and aptychus is **Hecticoceras hecticum**, and since the name of the type species of the important aptychus genus *CORNAPTYCHUS* thus becomes suppressed, confusion arises as to the nomenclature of numerous described species which are now assigned to *CORNAPTYCHUS*. The problem can be solved by ruling that this and other aptychus genera are to be classed as parataxa.

11. This paragraph deals with synonymy involving subjective indentification of the type species of aptychus genera with species of conchs other than the type species of the genera to which they are assigned. Seven genera of aptychi are contained in this group.

(1) *Trigonellites* (1811), *Aptychus* (1831), *Muensteria* (1835), and *LAMELLAPTYCHUS* (1927) versus **Oppelia** (1869). The four above-named aptychus genera are all either objective or subjective synonyms of each other, all having type species which are objectively or subjectively synonymous. This species has been subjectively identified with the aptychus in three conch-and-aptychus associations in which the conch has been subjectively identified as respectively **Oppelia flexuosa** (van Buch) 1831 ; **O. discus** (Quenstedt) 1856 ; and **O. euglypta** (Oppel) 1863. According to the ordinary operation of the *Règles*, the oldest aptychi names have priority over the conch names. According to our recommendations, as detailed in para. 5 above :

(a) the name *Trigonellites* Parkinson, 1811 [**T. lamellosus* Parkinson, 1811] should be suppressed under the Plenary Powers (see para. 4(3) above) ;

(b) the name *Aptychus* von Meyer, 1831 [**A. imbricatas* von Meyer, 1831 (=*Trigonellites lamellosus* Parkinson, 1811)] should be suppressed under the Plenary Powers ;

(c) the name *Muensteria* Eudes-Deslongchamps, 1835 [**M. sulcata* Eudes-Deslongchamps, 1835 (=*Trigonellites lamellosus* Parkinson, 1811)] should be suppressed as a long-overlooked name ;

(d) the name *LAMELLAPTYCHUS* Trauth, 1927 [**Trigonellites lamellosus* Parkinson, 1811] should, together with its type species, be ruled a parataxon, available for classification of aptychi but not for conchs.

(2) *Ichthyosiagones* (1822) versus **Aspidoceras** (1867). *Ichthyosiagones* Bourdet de la Nievre, 1822 [**Tellinites problematicus* Schlotheim, 1820 ; =*Trigonellites latus* Parkinson, 1811 (=*Laevaptychus latus*)]. Based on conch-and-aptychus associations, subjective synonyms among conchs include **Aspidoceras cyclotum** (Oppel) 1865 ; **A. inflatum** (Zieten) 1830 ; and **A. longispinum** (Sowerby) 1825. *Ichthyosiagones* has priority over **Aspidoceras** according to normal operation of the *Règles*, but we recommend its suppression as a long-overlooked name ;

(3) *GRANULAPTYCHUS* (1927) versus **Hlawiceras** (1921). *GRANULAPTYCHUS* Trauth, 1927 [**G. suevicus* Trauth, 1927]. The type specimen of the type species occurs in a conch identified as **Hlawiceras** sp. aff. **H. suevicum** (Wetzel) 1911. The type species of **Hlawiceras** Buckman, 1921, is ***H. platyrrymum** Buckman, 1921. The generic name based on the aptychus is junior to that based on the conch, which is not definitely identified specifically. We recommend that *GRANULAPTYCHUS* should be recognized as a parataxon, thus removing any conflict with **Hlawiceras.**

(4) *NEOANAPTYCHUS* (1931) versus **Gaudryceras** (1894). *NEOANAPTYCHUS* Nagao, 1931 [**N. tenuiliratus* Nagao, 1931]. Holotype of the type species in living chamber of a conch identified as **Gaudryceras tenuiliratum** Yabe, 1903, Cretaceous of Japan. The genus **Gaudryceras** de Grossouvre, 1894, is based on ***Ammonites mitis** Hauer, 1866, as type species. Both the generic and specific names for the conch have priority over those applied to the aptychus, but we recommend that both the generic and specific names of the aptychus should be recognized as parataxa.

12. This paragraph is concerned with synonymy involving subjective identifications of the type species of conch genera with species of aptychi other than the type species of the genera to which they are assigned. The aptychus names given between quotation marks are as written by the author who identified the conch-aptychus association. The names given in square brackets are those which would be adopted as those of parataxa if our recommendations are approved. Only in this case is conflict avoided between the name of conch and aptychus. Conflicts with *SIDETES* are cited in subparagraph (1) to (5) ; with *CORNAPTYCHUS* in (6) to (8) ; with *LAMELLAPTYCHUS* in (9) to (11) ; with *GRANULAPTYCHUS* in (12) ; with *LAEVAPTYCHUS* in (13) to (15) ; and with *PRAESTRIAPTYCHUS* in (16).

(1) **Amaltheus** (1808) versus *SIDETES* (1847). **Amaltheus** de Montfort, 1808 (***A. margaritatus** Montf., 1808). Subj. syn., "*Anaptychus margaritatus*" Schlumberger, 1868 [=*SIDETES pala* (Trauth, 1935)]. The generic name *SIDETES* Giebel, 1847, and specific name *pala* Trauth, 1935, are both junior to the names applied to the conch.

(2) **Asteroceras** (1867) versus *SIDETES* (1847). **Asteroceras** Hyatt, 1867 [***Ammonites stellaris** Sowerby, 1815 ; subsequent selection Buckman, 1911 ; already placed on the *Official List* (ICZN *Opinion* 324)]. Subj. syn., "*Anaptychus sellaeformis bicarinata*" Trauth, 1935 [=*SIDETES sellaeformis bicarinatus* (Trauth)]. The generic name **Asteroceras** is junior to *SIDETES* Giebel, 1847, but the specific name **stellaris** is senior to Trauth's name for the aptychus found associated with the conch identified as **Asteroceras stellare.**

(3) **Pleuroceras** (1867) versus *SIDETES* (1847). **Pleuroceras** Hyatt, 1867 [***Ammonites spinatus** Bruguière, 1789 ; subsequent selection Fischer, 1882 ; already placed on the *Official List* (ICZN *Opinion* 324)] [=**Paltopleuroceras** Buckman, 1895 (obj.)]. Subj. syn., "*Anaptychus mitraeformis*" Trauth, 1935 [=*SIDETES mitraeformis* (Trauth)], and "*A. pala*" Trauth, 1935 [=*SIDETES pala* (Trauth)], both identified in the living chamber of conchs identified as **Ammonites spinatus** [=**Pleuroceras spinatum**]. The generic and specific names for the aptychi are junior to those of the conch.

(4) **Psiloceras** (1867) versus *SIDETES* (1847). **Psiloceras** Hyatt, 1867 [***Ammonites planorbis** Sowerby, 1824 ; subsequent selection Spath, 1924] ; already placed on the *Official List* (ICZN *Opinion* 324) ; [=**Psilonoticeras** Quenstedt, 1883 (obj.)]. Subj. syn., "*Anaptychus carapax angusta*" Trauth, 1935 [=*SIDETES carapax angustus* (Trauth)]. Introduction of the designation "anaptychus" by Oppel (1856) was based on discovery of an undivided aptychus in the living chamber of a conch identified by him as **Ammonites planorbis** but plainly he used the word as a morphological term rather than as a generic name. Ascribed to *SIDETES* Giebel, 1847, the generic name for this aptychus is older than the generic name given to the conch but the specific name is junior to **planorbis.**

(5) **Arietites** (1869) versus *SIDETES* (1847). **Arietites** Waagen, 1869 [***Ammonites bucklandi** Sowerby, 1816, already placed on the *Official List* (ICZN *Opinion* 305)] [=**Arieticeras** Quenstedt, 1883 already placed on the *Official List of Rejected and Invalid Generic Names* (ICZN *Opinion* 337) (obj.) (*non* Sequenza, 1885, already placed on the *Official List*, ICZN *Opinion* 337)]. Subj. syn., "*Anaptychus sellaeformis*" Trauth, 1935 [=*SIDETES sellaeformis* (Trauth)]. The generic name *SIDETES* Giebel, 1847, is older than

Arietites but it could not appropriately be recognized as a replacement name for this genus of conchs because the type of aptychus to which it applies occurs with several other genera of conchs. We recommend that it be recognized as a parataxon, and thus be removed from competition with all the foregoing conch genera.

(6) **Hildoceras** (1867) versus *CORNAPTYCHUS* (1927). **Hildoceras** Hyatt, 1867 [***Ammonites bifrons** Bruguière, 1789; subsequent selection Buckman, 1889] already placed on the *Official List* (ICZN *Opinion* 304). Subj. syn., "*Aptychus elasma*" von Meyer, 1831 [=*CORNAPTYCHUS elasma* (Meyer)]. Trauth, 1936 (: 49–52) has identified this same species of *CORNAPTYCHUS* associated with conchs identified by him as **Leioceras opalinum** (Reinecke), **Pseudolioceras lythense** (Young & Bird), **P. falcatum** (Quenst.), **P. gigas** (Quenst.), and **P. lineatum** (Quenst.). Both the generic and specific names for the species of aptychus associated with **Hildoceras bifrons** are junior to the names of the conch.

(7) **Leioceras** (1867) versus *CORNAPTYCHUS* (1927). **Leioceras** Hyatt, 1867 [***Nautilus opalinus** Reinecke, 1818; subsequent selection Buckman, 1887]. Subj. syn. "*Aptychus elasma*" von Meyer, 1831 [=*CORNAPTYCHUS elasma* (Meyer)]. See preceding subparagraph for other reported associations of this aptychus with species of conchs.

(8) **Harpoceras** (1869) versus *CORNAPTYCHUS* (1927). **Harpoceras** Waagen, 1869 [***Ammonites falcifer** Sowerby, 1820; subsequent selection Arkell, 1951 already placed on the *Official List* (ICZN *Opinion* 303)] [**Falciferites** Breistroffer, 1947 (obj.)]. Subj. syn., "*Tellinites sanguinolarius*" Schlotheim, 1820 [*CORNAPTYCHUS sanguinolarius* (Schloth)], which is reported by Trauth, 1936, to occur also in association with conchs identified as **Harpoceras serpentinum** (Reinecke) 1818 and **Pseudolioceras lythense** (Young & Bird) 1828. The generic name for this aptychus is junior to names used for associated conchs. We recommend *CORNAPTYCHUS* be recognized as a parataxon, and thus removed from competition with these conch genera and those of the two preceding subparagraphs.

(9) **Oppelia** (1869) versus *LAMELLAPTYCHUS* (1927). **Oppelia** Waagen, 1869 [***Ammonites subradiatus** J. DeC. Sowerby, 1823; subsequent selection H. Duovillé, 1884; already placed on the *Official List* (ICZN *Opinion* 324)]. Subj. syn., "*LAMELLAPTYCHUS subradiatus*" Trauth, 1930 [=*LAMELLAPTYCHUS* sp.].

(10) **Haploceras** (1870) versus *LAMELLAPTYCHUS* (1927). **Haploceras** Zittel, 1870 [***Ammonites elimatus** Oppel in Zittel, 1868; subsequent

selection Spath, 1923]. Subj. syn., "*Aptychus beyrichi*" Oppel, 1865 [=*LAMELLAPTYCHUS beyrichi* (Oppel) Trauth, 1938]. This synonymization is based on the occurrence of the aptychus *L. beyrichi* in the living chamber of a conch identified as belonging to **Haploceras elimatum.** As reported above (para. 10, subpara. (1)), *PUNCTAPTYCHUS punctatus* has also been identified in association with a conch claimed to represent **Haploceras elimatum.** Clearly identification of conch or aptychus must be at fault in at least one of these conch-and-aptychus associations. If *LAMELLAPTYCHUS* and *PUNCTAPTYCHUS* are recognized as parataxa, such uncertainties of identification will not affect nomenclature.

(11) **Neochetoceras** (1925) versus *LAMELLAPTYCHUS* (1927). **Neochetoceras** Spath, 1925 [***Ammonites steraspis** Oppel, 1863]. Subj. syn., "*Aptychus rectecostatus*" Peters, 1854 [=*LAMELLAPTYCHUS rectecostatus* (Peters), Trauth, 1938] and "*Aptychus theodosia*" Deshayes, 1838 [=*LAMELLAPTYCHUS theodosia* (Deshayes) *longa* Trauth, 1938]. Seemingly, the conch or aptychus comprising one of the associated pairs is misidentified for, as in the previous subparagraph, it is unreasonable to suppose that a single species defined by characters of the conch would possess in different individuals aptychi classifiable as belonging to different species. In any case, however, we recommended that *LAMELLAPTYCHUS* be recognized as a parataxon, thus nullifying the effect that such subjective identifications would otherwise have on the nomenclature.

(12) **Lithacoceras** (1900) versus *GRANULAPTYCHUS* (1927). **Lithacoceras** Hyatt, 1900 [***Ammonites ulmensis** Oppel, 1858]. Subj. syn., "*Aptychus planulati*" Fraas, 1885 [=*GRANULAPTYCHUS planulati* (Fraas), Trauth, 1937]; both generic and specific names based on the conch are senior to those applied to the aptychus.

(13) **Physodoceras** (1900) versus *LAEVAPTYCHUS* (1927). **Physodoceras** Hyatt, 1900 [***Ammonites circumspinosus** Quenstedt, 1858]. Subj. syn., "*Trigonellites latus*" Parkinson, 1811 [=*LAEVAPTYCHUS latus* (Parkinson), Trauth, 1931]. The generic name based on the aptychus is junior to that based on the conch, whereas the specific name of the conch is junior to that applied to the aptychus.

(14) **Hybonoticeras** (1947) versus *LAEVAPTYCHUS* (1927). **Hybonoticeras** Breistroffer, 1947 [***Ammonites hybonotus** Oppel, 1863]. [obj. syn., **Waagenia** Neumayr, 1878 (*non* Kriechbaumer, 1874)]. Subj. syn., "*LAEVAPTYCHUS hybonotus*" Trauth, 1931 [=*LAEVAPTYCHUS* sp.]. The generic name based on the conch is junior to that based on the aptychus.

(15) **Euaspidoceras** (1931) versus *LAEVAPTYCHUS* (1927). **Euaspidoceras** Spath, 1931 [***Ammonites perarmatus** Sowerby, 1822]. Subj. syn., " *Trigonellites latus* " Parkinson, 1811 [=*LAEVAPTYCHUS latus* (Parkinson), Trauth, 1931]. Both generic and specific names of the conch species are junior to those of the aptychus species. We recommend recognition of *LAEVAPTYCHUS* as a parataxon, thus removing it from competition with the conch genera of this and the two preceding subparagraphs.

(16) **Sigaloceras** (1900) versus *PRAESTRIAPTYCHUS* (1927). **Sigaloceras** Hyatt, 1900 [***Ammonites calloviensis** Sowerby, 1815 ; subsequent designation (ICZN *Opinion* 324)]. Subj. syn., *PRAESTRIAPTYCHUS kostromensis* Trauth, 1930. Both the generic and specific names based on the aptychus are junior to those based on the conch. *PRAESTRIAPTYCHUS* is another of the names which we recommend should be recognized as a parataxon.

13. This paragraph concerns synonymy involving subjective identification of conch species and aptychus species, neither of which is the type species of a genus, the synonymy being based on associations of conchs with aptychi which are accepted as proof that respective pairs were derived from single ammonoids. It might be supposed that, by the application of the present *Règles*, such synonymization involved a straight competition between the two specific names in question, but this is, in fact, only the case when the type specimen of the aptychus species is concerned. Otherwise, as has been repeatedly emphasized, the characters of the aptychus species are insufficiently diagnostic to allow certain identification with a single conch species ; one aptychus species may belong to several different conch species. Thus, even in this, the simplest class of subjective synonyms that we have to deal with, the present *Règles* cannot be applied without ambiguity. Recognition of all the species based on aptychi as parataxa would result automatically from the general provisions we are here recommending, and would obviate all the above-mentioned ambiguities. Specimens illustrating conch-and-aptychus associations that involve species other than type species of genera are fairly numerous. We here list 21 genera and 43 species of conchs which occur with aptychi belonging to 8 genera and 36 species (including subspecies). These are cited as concisely as possible, grouped according to aptychus genera represented in the associations. The symbol " + " indicates " associated with ".

(1) Associations with species of *SIDETES* Giebel, 1847 [**S. striatus* Giebel, 1849] :

(a) **Lytoceras** Suess, 1865 [***Ammonites fimbriatus** Sowerby, 1817]. **L. cornucopia** (Young & Bird), 1822 + *S. latexcisus* (Trauth), 1935.

(b) **Arnioceras** Hyatt, 1867 [***A. cuneiforme** Hyatt, 1867]. **A. flavum** (Buckman), 1918 + *S. peltarion* (Trauth), 1935 [=*S. falcaries* (Quenst.), 1883].

(c) **Coroniceras** [***Ammonites kridion** Zieten, 1830]. **C. rotiforme** (Sowerby), 1824 + *S. carapax* (Trauth), 1935.

(d) **Psiloceras** Hyatt, 1867 [***Ammonites planorbis** Sowerby, 1824]. **P. harpoptychum** Holland, 1900 + *S. listron* (Trauth), 1935.

(e) **Mojsvarites** Pompeckj, 1895 [***Ceratites aginor** Münster, 1834]. **M. planorboides** (Gümbel), 1861 + *S. planorboides* (Gümbel), 1861.

(f) **Damesites** Matsumoto, 1942 [***Desmoceras damesi** Jimbo, 1894]. **D. semicostatus** (Yabe), 1927 + *S. semicostatus* (Nagao), 1932.

(2) Associations with species of *CORNAPTYCHUS* Trauth, 1927 [**Aptychus hectici* Quenstedt, 1849]:

(a) **Hildoceras** Hyatt, 1867 [***Ammonites bifrons** Bruguière, 1789.—(i) **H. kisslingi** Hug + *C. sanguinolarius* (Schlotheim) *accurvata-granulata* Trauth, 1936.—(ii) **H. levisoni** (Simpson) + *C. sanguinolarius* (Schlotheim) *levisoni* Trauth, 1936.

(b) **Harpoceras** Waagen, 1869 [***Ammonites falcifer** Sowerby, 1820].—(i) **H. exaratum** (Young & Bird), 1828 + *C. sanguinolarius sanguinolarius* (Schlotheim), 1820.—(ii) **H. serpentinum** (Reinecke), 1818 + *C. sanguinolarius* (Schlotheim) *sigmopleura* Trauth, 1936.—(iii) ? **H. falcifer** (Sowerby), 1820 + *C. sanguinolarius* (Schlotheim) *accurvata* Trauth, 1936.

(c) **Dumortieria** Haug, 1885 [***Ammonites levesquei** d'Orbigny, 1844]. **D. subundulata** (Branco) + *C. subundulatus* (Buckman), 1891.

(d) **Pseudolioceras** Buckman, 1889 [***Ammonites compactus** Simpson *in* Buckman, 1889].—(i) **P. lythense** (Young & Bird), 1828 + *C. sanguinolarius sanguinolarius* (Schlotheim), 1820.—(ii) **P. lythense** (1828) + *C. sanguinolarius* (Schlotheim) *sigmopleura* Trauth, 1936.—(iii) **P. lythense** (1828) + *C. elasma* (von Meyer), 1831. . . (iv) **P. lythense** (1828) + *C. ovatus* (von Meyer) *clathratus* Trauth, 1936.—(v) **P. sublythense** (Quenstedt) 1886 + *C. sublythensis* Trauth, 1936.—(vi) **P. falcatum** (Quenstedt), 1886 + *C. elasma* (von Meyer), 1831.—(vii) **P. gigas** (Quenst.), 1886 + *C. elasma* (1831).—(viii) **P. lineatum** (Quenst.), 1885 + *C. elasma* (1831).—(ix) **P. lineatum** (Quenst.), 1885 + *C. elasma* (von Meyer) *concordans* Trauth, 1936.

(3) Associations with species of *LAEVAPTYCHUS* Trauth, 1927 [**Aptychus meneghinii* Zigno, 1870]:

(a) **Aspidoceras** Zittel, 1868 [***Ammonites rogoznicensis** Zeuschner, 1846].—(i) **A. acanthicum** (Oppel), 1863 + *L. laevis longus* (von Meyer), 1831.—(ii) **A. bispinosum** (Zieten), 1830 + *L. laevis longus* (1831).—(iii) **A. inflatum** (Zieten), 1830 + *L. laevis longus* (1831).

(b) **Hybonoticeras** Breistroffer, 1947 [***Ammonites hybonotus** Oppel, 1863]. **H. autharis** (Oppel), 1863 + *L.* " *autharis* " (Oppel), 1863.

(4) Associations with species of *LAMELLAPTYCHUS* Trauth, 1927 [**Trigonellites lamellosus* Parkinson, 1811]:

(a) **Oppelia** Waagen, 1869 [***Ammonites subradiatus** J. De C. Sowerby, 1823].—(i) **O. bous** (Oppel), 1863 + *L. rectecostatus* (Peters), 1854.—(ii) **O. euglypta** (Oppel), 1863 + *L. rectecostatus*, 1854.—(iii) **O. hauffiana** (Oppel), 1863 + *L.* " *hauffianus* " (Oppel), 1863.—(iv) **O. trachynotus** (Oppel), 1863 + *L. pseudoparallelogramma* Trauth, 1938.—(v) **O. holbeini** (Oppel), 1863 + *L. sparsilamellosus* (Gümbel), 1861.—(vi) **O. lithographica** (Oppel), 1863 + *L.* " *lithographicus* " (Oppel), 1863.—(vii) **O. thoro** (Oppel), 1863 + *L.* " *thoro* " (Oppel), 1863.—(viii) **O. thoro** (1863) + *L.* " *thoro* " (Oppel) *laevadsymphysalis* Trauth, 1938.—(ix) **O. flexuosa hastata** (Quenstedt), ?1885 + *L.* " *thoro* " (Oppel), 1863.—(x) **O. haberleini** (Oppel), 1863 + *L.* " *haberleini* " (Oppel), 1863.

(b) **Haploceras** Zittel, 1870 [***Ammonites elimatus** Oppel *in* Zittel, 1868].—(i) **H. lingulatum solenoides** (Hohenegger), 1861 + *L. rectecostatus* (Peters), 1854.—(ii) **H. lingulatum solenoides.** (1861) + *L. lamellosus* (Parkinson) " *solenoides* " Trauth, 1938.

(5) Associations with species of *PRAESTRIAPTYCHUS* Trauth, 1927 [**P. gerzensis* Trauth, 1930]:

(a) **Perisphinctes** Waagen, 1869 [***Ammonites variocostatus** Buckland, 1836].—(i) **P. fasciferus** Neymayr, 1873 + *P. fraasi* Trauth, 1937.—(ii) **P. siliceus** (Quenstedt), 1856 + *P. fraasi* (1937).

(b) **Parkinsonia** Bayle, 1878 [***Ammonites parkinsoni** Sowerby, 1821]. **P. subarietis** Wetzel, 1911 + *P. subarietis* Trauth, 1930.

(c) **Sphaeroceras** Bayle, 1878 [***Ammonites brongniarti** Sowerby, 1817]. **S. bullatum** (Reynes), 1867 + *P.* " *bullatus* " Trauth, 1930.

(6) Associations with species of *STRIAPTYCHUS* Trauth, 1927 [**S. spinigeri* Trauth, 1927]:

(a) **Acanthoscaphites** Nowak, 1911 [***Scaphites tridens** Kner, 1848]. **A. roemeri** (Schlüter), 1872 + *S. roemeri* Trauth, 1927.

(b) **Discoscaphites** Meek, 1876 [***Scaphites conradi** Morton, 1834]. **D. cheyennensis** (Owen), 1852 + *S. cheyennensis* Trauth, 1927.

(c) **Worthoceras** Adkins, 1928 [***Macrocephalites platydorsus** Scott, 1924]. **W. bladenensis** (Schlüter), 1871 + *S. schlueteri* Trauth, 1927.

(7) Associations with species of *GRANULAPTYCHUS* Trauth, 1927 [**G. suevicus* Trauth, 1930]:

(a) **Lithacoceras** Hyatt, 1900 [***Ammonites ulmensis** Oppel, 1858].—(i) **L. rueppellianus** (Quenstedt), 1888 + *G. planulati* (Fraas), 1855.—(ii) **L. filiplex** (Quenst.), 1888 + *G. planulati* (1855).—(iii) **L. eudichotomus** Zittel, 1870 + *G. planulati* (Fraas), 1855.

(8) Associations with species of *LAEVILAMELLAPTYCHUS* Trauth, 1930 [**Trigonellites ceratoides* Ooster, 1857]:

(a) **Pseudolissoceras** Sapth, 1925 [***Neumayria zitteli** Burckhardt, 1903].—(i) **P. aporum** (Oppel), 1863 + *L.* "*aporus*" Trauth, 1931.—(ii) **P. zitteli** (Burckhardt), 1903 + *L.* "*zitteli*" Trauth, 1937.

14. The recommendations which we now submit are that the International Commission should:

(1) direct that all names introduced for taxa whose types are, in the opinions of the authors, aptychi of cephalopods are to be in terms of parataxa, and as such are not available as the names of taxa based on ammonoid conchs, or conch-and-aptychus associations;

(2) use the Plenary Powers to suppress the following long-overlooked generic names (all of the Class Cephalopoda) (some of which are, as indicated, also *nomina dubia*) for the purposes of the Law of Priority but not for those of the Law of Homonymy:

Tellinites Schlotheim, 1813;
Solennites Schlotheim, 1813;
Ichthyosiagones Bourdet de la Nievre, 1822;
Muensteria Eudes-Deslongchamps, 1835;

(3) use the Plenary Powers to suppress the generic names *trigonellites* Parkinson, 1811, *Aptychus* von Meyer, 1831 and *Palanaptychus* Trauth, 1927 (all of the Class Cephalopoda) for the purposes of the Law of Priority but not for those of the Law of Homonymy ;

(4) use the Plenary Powers to direct that species described under the following names are to be attributed to the authors named, and are to be referred to the type specimen which formed the original of the figure named in parenthesis (or as otherwise defined below) which is to be regarded as the holotype of the species in question :

GRANULAPTYCHUS suevicus Trauth, 1930 (*op. cit.* : 389, pl. 5, fig. 4) ;

PSEUDOSTRIAPTYCHUS pseudostobaei Trauth, 1928 (unfigured specimen from Coesfelder Berge, Germany, described by Schlüter, 1872, *op. cit.* : 58 and Trauth, 1928, *op. cit.* : 168) ;

NEOANAPTYCHUS tenuiliratus Nagao, 1931 (*op. cit.* : 109, fig. 1) ;

(5) use the Plenary Powers to direct that the following species although not stated by the authors to be names of aptychi, are nevertheless to be regarded as names of parataxa, available only to designate species of aptychi :

Trigonellites lamellosus Parkinson, 1811 ;
Trigonellites ceratoides Ooster, 1857 ;

(6) place the under-mentioned generic names (all Class Cephalopoda, Order Ammonoidea) on the *Official List of Generic Names in Zoology* as parataxa, available only for designation of ammonoid aptychi and not for ammonoid conchs or conch-and-aptychus associations :

(a) *SIDETES* Giebel, 1847 (*N. Jahrb. Mineralogie*, **1847** : 821) (gender : masculine) (type species, by monotypy : *Sidetes striatus* Giebel, 1849, *Deutsche geol. Gesell.* **1** : 99, text-fig. p. 100) ;

(b) *CORNAPTYCHUS* Trauth, 1927 (*Ann. naturhist. Mus. Wien*, **41** : 189, 228) (gender : masculine) (type species, by monotypy : *Aptychus hectici* Quenstedt, 1849, *Petrefactenkunde Deutsch.* **1** : 119, 315, pl. 8, fig. 10) ;

(c) *CRASSAPTYCHUS* Trauth, 1927 (*op. cit.* : 205, 228) (gender : masculine) (type species, by monotypy : *Aptychus crassus* Hébert, 1855, *Mém. Soc. géol. France* **5** : 368, pl. 28, fig. 5a, b) ;

(d) *GRANULAPTYCHUS* Trauth, 1927 (*op. cit.* : 217, 228) (gender: masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L.** *GRANULAPTYCHUS suevicus* Trauth, 1930, *op. cit.* : 389, pl. 5, figs. 3–5) [species defined under Plenary Powers under subpara. (4) above];

(e) *LAEVAPTYCHUS* Trauth, 1927 (*op. cit.* : 189, 228) (gender : masculine) (type species, by monotypy : *Aptychus meneghinii* Zigno, 1870, *Mem. reale Ist. Venete Sci. Arte* **15** : 11, pl. 8, figs. 1–4);

(f) *LAMMELLAPTYCHUS* Trauth, 1927 (*op. cit.* : 189, 228) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Trigonellites lamellosus* Parkinson, 1811, *Orangic remains former world*, **3** : 184, pl. 13, figs. 10–11) [species defined under Plenary Powers under subpara. (5) above];

(g) *LISSAPTYCHUS* Trauth, 1927 (*op. cit.* : 220, 231) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Aptychus leptophyllus* Sharpe, 1856, *Mon. Palaeontogr. Soc.* : 55, pl. 25, figs. 1a, b);

(h) *PALAPTYCHUS* Trauth, 1927 (*op. cit.* : 214) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Aptychus carbonarius* Koenen, 1879, *N. Jahrb. Mineralogie* **1879** : 317);

(i) *PRAESTRIAPTYCHUS* Trauth, 1927 (*op. cit.* : 219, 230) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Praestriaptychus gerzensis* Trauth, 1930, *op. cit.* : 380, pl. 5, figs. 14–15);

(j) *PSEUDOSTRIAPTYCHUS* Trauth, 1927 (*op. cit.* : 219, 230) (gender : masculine) (type species, by monotypy, *Pseudostriaptychus pseudostobaei* Trauth, 1928, *op. cit.* : 165) (species defined under Plenary Powers under subpara. (4) above);

(k) *PTERAPTYCHUS* Trauth, 1927 (*op. cit.* : 188, 218) (gender : masculine) (type species, by monotypy, *Aptychus numida* Coquand, 1854, *Mém. Soc. géol. France* (2) **5** : 140, 148, pl. 3, fig. 1);

(l) *PUNCTAPTYCHUS* Trauth, 1927 (*op. cit.* : 200, 228) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Aptychus punctatus* Voltz, 1837, *N. Jahrb. Mineralogie*, **1837** : 435, Trauth, 1935, *Jahrb. geol. Bundesanst. Wien* **75** : 315, pl. 12, figs. 1–6);

(m) *RUGAPTYCHUS* Trauth, 1927 (*op. cit.* : 220, 228) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Aptychus rugosus* Sharpe, 1856, *Mon. Palaeontogr. Soc.* : 57, pl. 24, figs. 8–9) ;

(n) *SPINAPTYCHUS* Trauth, 1927 (*op. cit.* : 200, 220) (gender : masculine) (type species, by monotypy : *Aptychus spinosus* Cox, 1926, *Ann. Mag. nat. Hist.* (9) **17** : 577, pl. 24, figs. 1–3) ;

(o) *STRIAPTYCHUS* Trauth, 1927 (*op. cit.* : 189, 229) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.* **L** : *Striaptychus spinigeri* Trauth, 1927, *op. cit.* : 244 ; Schlüter, 1872, *Palaeontogr.* **21** : 82, pl. 25, figs. 5–7) ;

(p) *LAEVILAMELLAPTYCHUS* Trauth, 1930 (*Ann. naturhist. Mus. Wien* **44** : 336) (gender : masculine) (type species, by subsequent selection, Moore & Sylvester-Bradley, 1956, *Treatise Invert. Paleont.*, **L** : *Trigonellites ceratoides* Ooster, 1857, *Petrif. remarq. Alpes Suisses*, (2) : 16, pl. 6, figs. 6–8, pl. 7, fig. 19) [species defined under Plenary Powers under subpara. (5) above] ;

(q) *NEOANAPTYCHUS* Nagao, 1931 (*Proc. Imp. Acad. Japan* **7** : 109) (gender : masculine) (type species, by monotypy : *Neoanaptychus tenuiliratus* Nagao, 1931, *op. cit.* : 109, figs. 1–2) [species defined under Plenary Powers under subpara. (4) above] ;

(7) place the under-mentioned specific names on the *Official List of Specific Names in Zoology* as parataxa, available only for designation of aptychi :

(a) *carbonarius* Koenen, 1879, as published in the combination *Aptychus carbonarius* (specific name of type species of *PALAPTYCHUS* Trauth, 1927) ;

(b) *ceratoides* Ooster, 1857, as published in the combination *Trigonellites ceratoides* (specific name of type species of *LAEVILAMELLAPTYCHUS* Trauth, 1930) (classed as a parataxon under the Plenary Powers as in subpara. (5) above) ;

(c) *crassus* Hébert, 1855, as published in the combination *Aptychus crassus* (specific name of type species of *CRASSAPTYCHUS* Trauth, 1927) ;

(d) *gerzensis* Trauth, 1930, as published in the combination *PRAESTRIAPTYCHUS gerzensis* (specific name of type species of *PRAESTRIAPTYCHUS* Trauth, 1927) ;

(e) *hectici* Quenstedt, 1849, as published in the combination *Aptychus hectici* (specific name of type species of *CORNAPTYCHUS* Trauth, 1927) ;

(f) *lamellosus* Parkinson, 1811, as published in the combination *Trigonellites lamellosus* (specific name of type species of *LAMELLAPTYCHUS* Trauth, 1927) (classed as a parataxon by virtue of Plenary Powers as in subpara. (5) above) ;

(g) *leptophyllus* Sharpe, 1857, as published in the combination *Aptychus leptophyllus* (specific name of type species of *LISSAPTYCHUS* Trauth, 1927) ;

(h) *meneghinii* Zigno, 1870, as published in the combination *Aptychus meneghinii* (specific name of type species of *LAEVAPTYCHUS* Trauth, 1927) ;

(i) *numida* Coquand, 1854, as published in the combination *Aptychus numida* (specific name of type species of *PTERAPTYCHUS* Trauth, 1927) ;

(j) *pseudostobaei* Trauth, 1928, as published in the combination *PSEUDOSTRIAPTYCHUS pseudostobaei* (specific name of type species of *PSEUDOSTRIAPTYCHUS* Trauth, 1927) and defined by Plenary Powers as in subpara. (4) above ;

(k) *punctatus* Voltz, 1837, as published in the combination *Aptychus punctatus* (specific name of type species of *PUNCTAPTYCHUS* Trauth, 1927) ;

(l) *rugosus* Sharpe, 1857, as published in the combination *Aptychus rugosus* (specific name of type species of *RUGAPTYCHUS* Trauth, 1927) ;

(m) *spinigeri* Trauth, 1927, as published in the combination *STRIAPTYCHUS spinigeri* (specific name of type species of *STRIAPTYCHUS* Trauth, 1927) ;

(n) *spinosus* Cox, 1926, as published in the combination *Aptychus spinosus* (specific name of type species of *SPINAPTYCHUS* Trauth, 1927) ;

(o) *striatus* Giebel, 1849, as published in the combination *SIDETES striatus* (specific name of type species of *SIDETES* Giebel, 1847) ;

(p) *suevicus* Trauth, 1930, as published in the combination *GRANULAPTYCHUS suevicus* (specific name of type species of *GRANULAPTYCHUS* Trauth, 1927) and defined under Plenary Powers under subpara. (4) above ;

(q) *tenuiliratus* Nagao, 1931, as published in the combination *NEOANAPTYCHUS tenuiliratus* (specific name of type species of *NEOANAPTYCHUS* Nagao, 1931) and defined under Plenary Powers under subpara. (4) above ;

(8) place the under-mentioned generic names on the *Official Index of Rejected and Invalid Generic Names in Zoology*, all suppressed for the purposes of the Law of Priority but not for those of the Law of Homonymy under the Plenary Powers as in subparas. (2) and (3) above :—

Trigonellites Parkinson, 1811 ;
Solennites Schlotheim, 1813 ;
Tellinites Schlotheim, 1813 ;
Ichthyosiagones Bourdet de la Nièvre, 1822 ;
Muensteria Eudes-Deslongchamps, 1835 ;
Aptychus von Meyer, 1831 ;
Palanaptychus Trauth, 1927 ;

(9) place the under-mentioned generic names on the *Official Index of Rejected and Invalid Generic Names in Zoology* both for the purposes of the Law of Priority and those of the Law of Homonymy :—

Aptycus Deshayes, 1845 (invalid subsequent spelling of *Aptychus*) ;
Ichthyosiagon Herrmannsen, 1847 (invalid subsequent spelling of *Ichthyosiagones*) ;
Aptichus Crosse, 1867 (invalid subsequent spelling of *Aptychus*) ;
Anaptychus Schlumberger, 1868 (homonym of *Anaptychus* Stimpson, 1860) ;
Laevicornaptychus Trauth, 1936 (*nomen nudum*, proposed without indication of type species) ;

(10) place the under-mentioned generic name on the *Official Index of Rejected and Invalid Generic Names in Zoology* for the purposes of the Law of Priority but not for those of the Law of Homonymy :—

Synaptychus Basse, 1952 (junior objective synonym of *Striaptychus* Trauth, 1927).

(11) direct that the name " anaptychus " as used by Oppel, 1856, and " synaptychus " as used by Fischer, 1882, are not to be regarded as the names of genera, but solely as morphological terms.

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DOCUMENT 1/4

Editorial Note : The following paper by Dr. W. J. Arkell was published in 1954 (*Bull. zool. Nomencl.* 9 : 266—269), the intention of the applicant being to secure that names based solely upon aptychi should not be permitted to cause inconvenience and confusion in normal ammonite nomenclature. It was not concerned in any way with the question whether provisions should be inserted in the *Règles* regulating the nomenclature of discrete fragments of fossils, a matter which had not at that time been placed before the International Commission. Following the publication of Dr. Arkell's paper objection was raised on the ground that the adoption of a *Declaration* in the sense proposed might prejudice the general question of the recognition of names given to unidentifiable discrete fragments of fossils. For this reason Dr. Arkell's application was deferred for consideration in connection with the foregoing wider problem. Of the three papers submitted by Professor Moore and Mr. Sylvester-Bradley in regard to that problem the third, which is specially concerned with the naming of ammonite aptychi—here published as Document 1/3—is described by its authors (paragraph 1) as being, in part, a counter-proposal to that previously submitted by Dr. Arkell. The latter author's original application has accordingly been incorporated in the London Agenda Paper and is here republished (as Document 1/4) in immediate juxtaposition to the counter-proposal (Document 1/3) submitted by Professor Moore and Mr. Sylvester-Bradley. [intl'd. F.H. 11th July 1957.]

Proposed adoption of a " Declaration " that a generic or specific name based solely upon the " aptychus " of an ammonite (Class Cephalopoda, Order Ammonoidea) be excluded from availability under Article 27 of the " Règles " and proposed suppression of certain such names under the Plenary Powers.

By **W. J. ARKELL,** M.A., D.Sc., F.R.S.
(*Cambridge University, Sedgwick Museum, Cambridge*)

(Commission Reference : Z.N.(S.) 1056, formerly Z.N.(S.) 589 and 858)

Sub-section (*a*) of Article 27 of the *Règles* provides that " the oldest available name is retained when any part of an animal is named before the animal itself ". The present application seeks a clarification of this provision in one particular.

2. Many nominal genera are based upon nominal species of which the type specimens are incomplete. The foregoing rule is therefore, in general, desirable.

3. There are however certain special cases where the application of the above rule would lead to highly undesirable disturbance of existing nomenclature. Such cases can be dealt with either by the insertion of words in Article 27 ruling out from availability names based exclusively upon some specified part of an animal or can be eliminated individually by the names concerned being suppressed by the International Commission on Zoological Nomenclature under its Plenary Powers, the names so suppressed being then placed on the appropriate *Official Index of Rejected and Invalid Names in Zoology.*

4. An example of the undesirable disturbance of existing nomenclature which would arise from the strict application of Sub-section (*a*) of Article 27 is provided by the names bestowed upon the aptychi of ammonites, structures which are now generally admitted to be opercula, analogous with those of gastropods. In the first half of the XIXth century, the nature of these structures was not understood, and several nominal genera and nominal species were established for them in the belief, usually, that they were lammellibranchs. Subsequently, some of these opercula have been found *in situ* in the body-chamber of ammonites. In these cases the names currently used for the genera and species of ammonite concerned were not published until long after the names published for their aptychi. The names published for the aptychi being at present available names, there is a serious risk of disturbance in current nomenclatorial practice unless the International Commission on Zoological Nomenclature takes preventive action.

5. One of the oldest nominal genera based upon aptychi is *Trigonellites* Parkinson, 1811 (*Organic Remains former World* : 184), for which a description and good figures were provided by Parkinson. No type species was designated for this genus, the type species of which must however be one or other of the two originally included nominal species, *Trigonellites latus* Parkinson, 1811 (: 186, pl. 13, figs. 9, 12) and *T. lamellosus* Parkinson, 1811 (: 186, pl. 13, figs. 10, 11). Since Parkinson's time, the nominal species *T. latus* has been identified as having been based upon the aptychus of a species of the genus *Aspidoceras* Zittel, 1868, and *T. lamellosus* as having been based upon a species of the genus *Oppelia* Waagen, 1869. Thus, whichever of the foregoing species were to be selected as the type species of the nominal genus *Trigonellites*

Parkinson, a serious situation would arise, for in the one case the name *Trigonellites* Parkinson would replace the name *Aspidoceras* Zittel, while in the other case that name would replace *Oppelia* Waagen. These are both important genera and are the type genera of families ; the supersession of either of these names would give rise to confusion and would be open to strong objection. It is the object of the present application to prevent these and other names in current use from being invalidated by the resuscitation of these old names based upon aptychi. In the present case it is desirable that, as part of its decision in regard to the name *Trigonellites* Parkinson, the International Commission should place the name *Aspidoceras* Zittel on the *Official List of Generic Names in Zoology*. In the case of *Oppelia* Waagen, 1869, a proposal for its addition to the *Official List* has already been submitted to the International Commission (1951, *Bull. zool. Nomencl.* 2(6–8) : 227) and no further action is therefore needed here[1]. It may be noted that Dr. F. Trauth (Vienna), the sole world authority on aptychi, does not recognise as an available name any generic name or specific name based solely upon the aptychi of ammonites. See Trauth, F., 1927–1936, Aptychenstudien I–VIII (*Ann. naturh. Mus. Wien* **41–48**) (especially "Aptychenstudien I ", published in 1927 (*loc. cit.* **41** : 221–228)). It will be seen therefore that the action now recommended is in line with current usage both from the point of view of the study of ammonites and from that of the study of aptychi.

6. It is accordingly recommended that the International Commission should render a " Declaration " recommending the International Congress of Zoology to amend Article 27 of the *Règles* in such a way as to deprive of availability in zoological nomenclature any name based solely upon the aptychus of an ammonite.

7. The amendment of the *Règles* in the foregoing sense would completely dispose of the problem here under consideration, but in the nature of the case this is a remedy which cannot be secured until the suggested *Declaration* is reported to, and approved by, the next International Congress of Zoology. It would however be most undesirable that the particular names with which we are here concerned should be permitted to retain their present status until the next Congress, for, as matters now stand, it would otherwise be necessary to take account of them in the forthcoming *Treatise on Invertebrate Paleontology*. It is therefore recommended that the immediate situation should be dealt with by the suppression of these names by the International Commission under its Plenary Powers, the names in question, when so suppressed, being placed on the *Official Index of Rejected and Invalid Generic Names in Zoology*.

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[1] This name was later placed on the *Official List of Generic Names in Zoology* in *Opinion* 324 (published in 1955).

8. The proposals which are now specifically submitted to the International Commission on Zoological Nomenclature are that it should :—

(1) render a " Declaration " recommending that Sub-section (a) of Article 27 be amended by the addition of the following words excluding from its scope any generic name or specific name based solely upon the aptychus of an ammonite : " save that, where a nominal genus or nominal species of ammonites (Class Cephalopoda, Order Ammonoidea) has been established solely upon an aptychus or upon aptychi, the generic name or, as the case may be, the specific name published for the nominal genus or nominal species so established is to have no status in zoological nomenclature " ;

(2) in anticipation of the insertion in the *Règles* of the foregoing amendment of Article 27, use its Plenary Powers to suppress the under-mentioned names of generic and specific names of species, each of which is based solely upon the aptychus, or upon the aptychi, of ammonites :—

(a) the under-mentioned generic names :—

(i) *Trigonellites* Parkinson, 1811, *Organic Remains former World* **3** : 184 ;

(ii) *Solennites* Schlotheim, 1813, *Tasch. Min.* : 105 ;

(iii) *Solenites* Schlotheim, 1820, *Petref.* : 180 (an emendation of *Solennites* Schlotheim, 1813) ;

(iv) *Aptychus* Meyer, 1831, *Jahrb. f. Min.* 1831 : 393 ; *id.*, 1831, *N. Acta Acad. Caes. Leopold. Car.* **15** (No. 2) : 125 ;

(v) *Aptycus* Deshayes, 1845, *in* Lamarck, *Hist. Anim. sans Vertèbr.* (ed. 2) **11** : 228 (an emendation of *Aptychus* Meyer, 1831) ;

(vi) *Muensteria* Eudes-Deslongchamps, 1835, *Mém. Soc. linn. Normandie* **5** : 61 ;

(b) the under-mentioned specific names :—

(i) *lamellosus* Parkinson, 1811, as published in the binominal combination *Trigonellites lamellosus* ;

(ii) *latus* Parkinson, 1811, as published in the binominal combination *Trigonellites latus* ;

(3) place the six generic names proposed, under (2) (a) above, to be suppressed under the Plenary Powers, on the *Official Index of Rejected and Invalid Generic Names in Zoology* :

(4) place the two specific names proposed, under (2) (b) above, to be suppressed under the Plenary Powers on the *Official Index of Rejected and Invalid Specific Names in Zoology* ;

(5) place the generic name *Aspidoceras* Zittel, 1868 (*Pal. Mitt. Mus. Bayer.* **2** (Abt. 1) : 116) (gender of generic name : neuter) (type species, by monotypy : *Ammonites rogoznikensis* Zeuschner, 1868 (*in* Zittel, *Pal. Mitt. Mus. Bayer.* **2** (Abt. 1) : 116, pl. 24, fig. 5) on the *Official List of Generic Names in Zoology* :

(6) place the specific name *rogoznikensis* Zeuschner, 1868, as published in the combination *Ammonites rogoznikensis* (specific name of type species of *Aspidoceras* Zittel, 1868) on the *Official List of Specific Names in Zoology*.

DOCUMENT 1/5

Comments received in 1954 on the proposal by Dr. W. J. Arkell for the adoption of a "Declaration" depriving names based solely upon the aptychi of ammonites of the status of availability for the purposes of zoological nomenclature (i.e. of whole-animal nomenclature in the terminology of the "Parataxa Plan")

By **FRANCIS HEMMING**, C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

(Commission Reference : Z.N.(S.) 1056, formerly Z.N.(S.) 589 and 858)

The publication in 1954 of the application by Dr. W. J. Arkell for the adoption of a *Declaration* depriving names based solely upon the aptychi of ammonites of the status of availability for the purposes of zoological nomenclature, i.e. for the purpose of what becomes "whole-animal nomenclature" in the terminology of the "parataxa" elicited three comments. These were from the following specialists :—

(a) Joshua L. Baily, Jr. (*San Diego, California, U.S.A.* (letter dated 27th November 1954)

(b) C. W. Wright (*London*) (letter dated 30th November 1954)

(c) P. C. Sylvester-Bradley (*University of Sheffield, England*) (letter dated 29th December 1954).

2. The communications so received from Dr. Baily and Mr. Wright are reproduced in Annexe 1 and 2 respectively to the present note. The letter received from Mr. Sylvester-Bradley was an objection based upon the apprehension that the issue of a *Declaration* in the sense recommended by Dr. Arkell might prejudice the later introduction into the *Règles* of provisions regulating the nomenclature of discrete fragments of fossils unidentifiable as parts of whole-animals. The communications so received from Mr. Sylvester-Bradley are not here reproduced, for they have since been superceded by the detailed treatment of this problem, both generally and with particular reference to

names based solely upon the aptychi of ammonites in the detailed papers subsequently submitted which are here reproduced as Documents 1/1 and 1/3 respectively.

ANNEXE 1 TO DOCUMENT 1/5

Comment by **JOSHUA L. BAILY, Jr.**

(*San Diego, California, U.S.A.*)

(Letter dated 27th November 1954)

On the basis of the information supplied by Dr. Arkell, I would recommend the suppression of the name *Trigonellites* Parkinson, as otherwise it would have to displace one of two universally used and understood names.

I do not, however, agree with Dr. Arkell that subsection (a) of Article 27 of the *Règles* be amended to exclude the Aptychus of Ammonites from its application. The rule is short and compact and semantically expressed, and to make an exception to it would complicate it unnecessarily. Simple rules that are easily understood and easily remembered are the best. If further cases parallel to this one should develop it would be better to deal with each one separately by suspending the rules, rather than by amending the rules with complicated exceptions for the purpose of anticipating emergencies which may not arise.

ANNEXE 2 TO DOCUMENT 1/5

Comment by **C. W. WRIGHT**

(*London*)

(Letter dated 30th November 1954)

As a specialist in Mesozoic Ammonoidea I should like to support strongly Dr. Arkell's proposal for a *Declaration* that generic or specific names of ammonite aptychi should have no validity in zoological nomenclature.

DOCUMENT 1/6

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **W. J. ARKELL,** M.A., D.Sc., F.R.S.
(*Sedgwick Museum, Cambridge University, Cambridge*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 20th June 1956)

I have read these documents and support them without reserve. The proposal for a special category of parataxa with its own special nomenclature is in my view the only satisfactory solution to an increasingly troublesome problem facing palaeontologists. I think the authors have made their case completely.

I am not qualified to comment on the First Supplemental Application (Conodonts), and my support must be understood as limited to approval of the Application as a general solution for the problem, and as applied in detail in the Second Supplemental Application (Aptychi). On the latter I have some small comments on matters of fact, which in no wise detract from my acceptance of the authors' main thesis.

Paragraph 5 : Status of Anaptychus Oppel, 1856, and Synaptychus Fischer, 1882. It does not seem to me to be satisfactorily demonstrated that these names were not considered as genera by their authors. Oppel in his 1856—8 book did not print any generic or specific names in italics, and Anaptychus was not printed in italics ; and it was listed in the " Register " at the end of the book, which contains no other entries but genera and species. Synaptychus was printed by Fischer in italics with initial capital, as for genera, and he used neither italics nor initial capitals for morphological terms.

Paragraphs 12 and 13 : The generic attributions given in these paragraphs require careful sifting before they could be regarded as reliable. For instance : Euaspidoceras referred to in paragraph 12(15) is an Oxfordian and Callovian subgenus and it is highly doubtful whether it can have been found associated with *Aptychus latus*, which is essentially Kimeridgian. This is unlikely to be the only case of misidentification.

DOCUMENT 1/7

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **DON L. FRIZZELL** and **HARRIET FRIZZELL**
(*Rolla, Missouri, U.S.A.*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 21st June 1956)

We wish to endorse most enthusiastically the Moore/Sylvester-Bradley application for the recognition of the special category of " parataxa " for fragmental fossils. We feel strongly that this special category is indispensable for progress in micropaleontology.

Moore and Sylvester-Bradley (paragraph 5) refer to our recent use of dual nomenclature applied to fossil holothurian remains (Univ. Missouri School of Mines & Metallurgy, Bull., Tech. Ser., No. 89, 1955). We would stress the fact that our decision to use a dual system of binominal nomenclature within the framework of the international code—essentially identical with Moore and Sylvester-Bradley's proposal for " taxa " and " parataxa "—was made quite independently (except for the literature cited in our Monograph). No other course seems possible to us, if confusion in nomenclature is to be avoided and if the vast preponderance of fossil material is to be made available for use in stratigraphy and paleozoology. The concept of " parataxa " certainly should be incorporated within the *Règles*, and an orderly system of " parataonomy " so expedited !

DOCUMENT 1/8

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **ELIANE BASSE**
(*Laboratoir de Géologie, Sorbonne, Paris*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 23rd June 1956)

Je vous envoie mon accord au sujet des propositions concernant la nomenclature zoologique sous l'égide de Professor R. Moore et P. C. Sylvester-Bradley.

DOCUMENT 1/9

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **J. BROOKES KNIGHT**
(*Smithsonian Institution, United States National Museum,
Washington, D.C., U.S.A.*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 1st July 1956)

May I endorse the proposal of Raymond C. Moore and P. C. Sylvester-Bradley that the International Commission of Zoological Nomenclature establish a special category called " parataxa " for the classification and nomenclature of discrete fragments or life stages of animals which are inadequate for identification of whole-animal taxa.

Let me emphasize these authors recommendation " that the provisions concerning parataxa should be limited to special groups of animals specifically defined for the purpose by the Commission ".

It is my opinion that the period of six months which they propose be allowed to elapse between the publication in the *Bulletin* of a proposal to the Commission and the vote taken by the Commission is far too short. What with delays in the world-wide distribution of the *Bulletin* by mail, in sorting out and distributing the matter at the subscribing institution, the likelihood that the *Bulletin* will not be called promptly to the attention of all interested in this or other matter in it coupled with the time needed to digest the proposal and get a letter back to the Commission, the entire interval of six months may well have passed. I urge one year as a minimum.

DOCUMENT 1/10

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **H. SCHMIDT**

(*Geologisch-Palaeontologisches Institut der Georg August-Universität, Gottingen, Germany*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 3rd July 1956)

Zum Antrag Raymond C. Moore & Sylvester-Bradley möchte ich meine grundsätzliche Zustimmung ausdrücken. Auch ich halte es für notwendig, dass die Parataxa nomenklatorisch von den Taxa getrennt werden, und dass Homonymie solchen gegenüber verhindert wird. Bezüglich Festellung von Synonymie sollte von seiten der I.R.Z.N. jedoch keine Vorschrift gemacht werden.

Zum Wortlaut der Recommendation (sub 2a) schlage ich vor, das Wort " fragments " durch " parts " zu ersetzen.

Begründung : Es gibt zahlreiche Namen fur fossile Fährten. Diese sind niemals " fragments " von Tierkörpern, sie überliefern uns aber die Form von Teilen der Körper und können unter solche gerechnet werden, besonders weil ja auch sonst viele Fossilien eigentlich nur Abdrücke (manchmal Pseudomorphosen eines Minerals) nach Teilen eines Tierkörpers sind. Die Aufzählung der " zoological entities " bei Moore & Sylvester-Bradley ist in Bezug auf Fährten und Spuren unvollständig. Ich vermute, dass beide Forscher, denen diese Begriffe natürlich nicht fremd sind, durch Weglassen derselben eine Vereinfachung anstrebten. Ich bin aber der Meinung, dass auch die Parataxa aus diesem Bereich berücksichtigt werden sollten. Namen für Tetrapoden-Fährten (Beispiel : *Chirotherium*) sollten einbezogen werden. In der Kategorie Organogene Spuren (Definition bei Krejci-Graf, *Senckenbergiana* **14**, 1932, p. 29) gibt es beispielsweise solche, die auf Enteropneusta bezogen werden ; eine neue

Untersuchung könnte (theoretisch!) finden, dass andere " Vermes " zugrundeliegen, und so müssten nacheinander mehrere Tierklassen durch die Commission eine Zulassung für Parataxa bekommen.

Ich hoffe, dass der vorliegende Antrag so modifiziert werden kann, dass spätere Schwierigkeiten in dieser Richtung vermieden werden. Eine bestimmte Formulierung in diesem Sinne vorzuschlagen habe ich nicht, möchte auch den Antragstellern nicht vorgreifen.

Des weiteren bitte ich zu bedenken, dass es nach dieser Regelung im Einzelfall oft unklar bleiben wird, ob ein Taxon oder ein Parataxon vorliegt. Deshalb sollte für die Zukunft empfohlen werden, dass Parataxa schon in der Namensform als solche kenntlich gemacht werden. Ich habe dazu einen Vorschlag gemacht (*Paläontologische Zeitschrift* 28, 1954, p. 3).

Zu " Supplement Application Conodonts " : In Table 1 sind zu dem von mir beschriebenen *Westfalicus integer* (vorher " Gnathodus ") einige Vermutungen dargestellt. Ich habe kürzlich Schritte unternommen, um neues Material zu beschaffen. In einigen Monaten werden besser begründete Aussagen wahrscheinlich möglich sein.

Zu " Supplement Application, Aptychi " : Die Göttinger Sammlung besitzt mehrere Aptychi (es ist üblich, in solchen Fällen das Wort "Anaptychus " nach Oppel, 1856 zu verwenden, aber diese Unterscheidung halte ich für unrichtig) *in situ* bei *Crickites holzapfeli*, darunter das Original zu Matern, *Senckenbergiana* 13, 1931, p. 160. Danach gehört *Spathiocaris koeneni* Clarke, 1884, hierher und wäre p. 11[1] und 15[1] noch zu nennen. P. 41[1], 7a : *carbonarius* Koenen, 1879, ist nach Trauth, *Aptychen des Paläozoikums*, 1934, p. 78, zu streichen, was ich bestätigen kann.

Translation of letter by H. Schmidt reproduced above

In regard to the application of Raymond C. Moore & Sylvester-Bradley I would like to express my approval. I too think it necessary that the parataxa should be nomenclatorially separated from the taxa, thereby avoiding homonymy. Regarding the establishment of synonymy there should be no directions on the part of the I.R.Z.N.

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[1] The page numbers here cited are to the pages of the typescript of this paper which was circulated by the authors to certain specialists. Of the pages cited page 11 is here reproduced on pages 42-43, page 15 on pages 45-46, and page 41(7)(a) on page 65.

As regards the wording of the Recommendation (subparagraph 2a) I recommend that the word " fragments " be replaced by " parts ".

Explanation : There are in existence countless names for fossile marks. These are never " fragments " of animal bodies, but they give us the form of sections of these bodies and can therefore be considered as such, especially as many fossiles are really only imprints (sometimes pseudomorphosia of a mineral) of parts of animal bodies. The enumeration of " zoological entities " by Moore & Sylvester-Bradley is incomplete in regard to tracks and marks. I suppose that the two researchers, to whom this concept will not be a strange one, want to simplify matters by omissions. I am, however, of the opinion that also the parataxa falling into this group should be considered. Names for Tetrapod tracks (Example : *Chirotherium*) should be included. In the category Organogene marks (Definition by Krejci-Graf, *Senckenbergiana* **14**, 1932, p. 29) there are some which refer to Enteropneusta ; a new examination could (theoretically) find them to be based on another " Vermes ", and so the Commission would have to admit several animal groups to the parataxa.

I hope that the application can be so modified that difficulties which might later arise in this connection may be avoided. I cannot make definite suggestions in this matter, and also do not wish to anticipate those responsible for this application.

I would also like you to bear in mind that this Ruling would not always make it clear whether a taxon or parataxon is under consideration. Therefore it should be recommended that parataxa should be distinguishable in the form of the name. I have made a suggestion to this effect (*Paläontologische Zeitschrift* **28**, 1954, p. 3).

In the First Supplemental Application (Conodonts), Table 1, I have added a few remarks to my description of *Westfalicus integer* (formerly " Gnathodus "). I have lately taken steps to get new material. In a few months' time I hope to be able to give a better reasoned account.

Re the Second Supplemental Application (Aptychi) : The Göttinger collection possesses several Aptychi (it is usual in such cases to use the word "Anaptychus " after Oppel, 1856, but I do not consider this correct) *in situ* by *Crickites holzapfeli*, among which is the original of Matern, *Senckenbergiana* **13**, 1931, p. 160. According to this *Spathiocaris koeneni* Clarke, 1884, belongs here and should be quoted on p. 11[2] and p. 15[2]. Page 41[2], 7a : *carbonarius* Koenen, 1879, according to Trauth, *Aptychen des Paläozoikums*, 1934, p. 78, should be omitted, which I can confirm.

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[2] See Footnote 1.

DOCUMENT 1/11

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **CURT TEICHERT**
(*Petroleum Geology Laboratory, United States Geological Survey, Denver, Colorado, U.S.A.*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 9th July 1956)

Dr. R. C. Moore and Mr. Sylvester-Bradley have sent me a copy of their "Application for a Declaration recognizing Parataxa, etc." and I wish to record whole-hearted support of their proposals. These are extremely well phrased and very logically conceived. I find it quite difficult to offer any worthwhile comments except to say that the suggested solution of the problems arising out of the naming of discrete fragments of animals strikes me as good common sense.

To the groups of fragments and life-stages listed by Moore and Sylvester-Bradley I would like to add the genera founded on cephalopod mandibles. For these objects, mainly of Mesozoic age, about 10 or 12 generic names are now available, but for most of them it is not known with certainty whether they are of nautiloid or of belemnoid affinities. Classification and treatment of this group as a parataxon as suggested by Moore and Sylvester-Bradley would greatly facilitate taxonomic work.

DOCUMENT 1/12

The Moore/Sylvester-Bradley " Parataxa " Proposals

(Commission Reference : Z.N.(S.) 1056)

Copy of a letter dated 18th July 1956 from R. C. MOORE (*University of Kansas*) to H. SCHMIDT (*Gottingen, Germany*) commenting on suggestions submitted by J. Brookes Knight (Document 1/9), H. Schmidt (Document 1/10) and C. Teichert (Document 1/11)

It is very helpful to have your discussion of application of parataxa to various groups and the recommendation that wording in the basic declaration should be broadly applicable to groups defined and approved by the Commission for use of parataxa. Thus, it has been suggested that not only for parts of organisms common as fossils but life stages of some organisms as in parasitology may advantageously be classified and named in terms of parataxa. These questions are subject for study and future decision. In order to avoid independent subjective procedures, it has been the view of Sylvester-Bradley and me that the responsibility for designating the areas in which parataxa may be used, should be in the hands of the Commission. This is endorsed by a number of others such as J. Brookes Knight and Curt Teichert. Teichert has raised the question of applying parataxa to nomenclature of " lebensspuren " and evidently he agrees with thinking expressed in your letter. Purposely, we submitted only two Supplemental Applications because it has seemed to us that the principles should receive consideration and I hope adoption before the effort to apply parataxa in various groups is taken up comprehensively.

I thank you for the specific suggestions made with reference to the Supplemental Applications. I shall look forward to seeing the publication concerning conodonts which you mention, and will study the suggested changes for the Application concerning aptychi.

DOCUMENT 1/13

The Moore/Sylvester-Bradley " Parataxa " Proposals

By **D. T. DONOVAN**
(*University of Bristol*)

(Commission Reference : Z.N.(S.) 1056)

(Letter dated 10th September 1956)

I have received from Professor R. C. Moore and Mr. P. C. Sylvester-Bradley a duplicated draft of an application submitted to the International Commission on Zoological Nomenclature in which they ask for " parataxa " to be recognized as a special category.

I am writing to say that as a palaeontologist, although not one who is primarily concerned with the groups of fossils for which the category " parataxon " is proposed, the concept seems to me to be a useful one, and I therefore support the proposal.

DOCUMENT 1/14

Note on the provision in the Moore/Sylvester-Bradley " Parataxa " Plan for the possible application of the " parataxa system " to the naming of collective groups of certain stages in the life histories of parasites.

By **FRANCIS HEMMING,** C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

(Commission Reference : Z.N.(S.) 1056)

On 8th October 1956 I received a letter dated 3rd October 1956 from Professor A. C. Walton, Secretary to the American Society of Parasitologists, transmitting a Resolution adopted by the Society commenting upon the relationship in the field of the nomenclature of parasites of the " Collective Group " Concept to that which (in Document 1/1) Professor Raymond C. Moore and Mr. P. C. Sylvester-Bradley had recommended should be recognised under the term " Parataxon ".

2. The receipt of the foregoing resolution led to correspondence between the Office of the Commission, Professor Walton, Dr. Allen McIntosh and Professor Raymond C. Moore. Later, Dr. McIntosh communicated to the Office of the Commission copies of three further documents, namely, a letter addressed by Dr. McIntosh to Dr. Norman R. Stoll, a letter addressed by Professor J. Chester Bradley to Professor Walton, and Dr. McIntosh's reply to Professor Bradley.

3. The Resolution adopted by the American Society of Parasitologists is attached to the present note as Annexe 1. The ensuing correspondence between the Office of the Commission and the specialists referred to in paragraph 2 above is attached as Annexes 2 to 6. The three documents communicated to the Office of the Commission by Dr. McIntosh are attached as Annexes 7 to 9.

ANNEXE 1 TO DOCUMENT 1/14

Resolution by the American Society of Parasitologists

(Transmitted by Professor A. C. Walton under cover of a
letter dated 3rd October 1956)

(a) That the " parataxa plan ", as outlined by Moore and Sylvester-Bradley, will not meet the needs of parasitologists.

(b) That the " Collective groups " have offered a suitable solution for dealing with the problem of nomenclature for larval stages of parasites, of unknown systematic position.

(c) That since there appears to be some difficulty among zoologists in the use of the Recommendation pertaining to " Collective groups " the Commission should clarify the Recommendation by a Declaration specifying the status of names associated with them, therefore the Society proposes the following resolution :

> Certain biological groups which have been proposed distinctly as collective groups, not as systematic units, may be treated for convenience as if they were genera, but they require no type species. Any specific name proposed in combination with a collective group name shall have the same status as if it had been proposed in combination with a generic name of a systematic unit.

ANNEXE 2 TO DOCUMENT 1/14

Copy of a letter dated 10th October 1956 from FRANCIS HEMMING (*London*) to A. C. WALTON (*American Society of Parasitologists*)

I should like, if I may, to raise a question of interpretation regarding the Resolution favoured by your Society. Am I right in thinking that it is the object of this Resolution to secure that where names or terms are published to distinguish collective groups and not to serve as systematic units the names or terms so published though ranking for priority with one another would not possess any status in zoological nomenclature as generic names, and would not preoccupy the names so used for use as generic names by later authors either in the same or in another part of the Animal Kingdom ?

ANNEXE 3 TO DOCUMENT 1/14

Copy of a letter dated 22nd October 1956 from ALLEN McINTOSH (*Chairman, Committee on Nomenclature, American Society of Parasitologists*) to FRANCIS HEMMING (*London*).

Your letter of 10th October 1956 to Dr. A. C. Walton, Secretary American Society of Parasitologists, Knox College, Galesburg, Illinois has been referred to me for reply to the question you have raised regarding the resolution presented by the A.S.P. pertaining to collective groups.

The purpose of the resolution is to request the Commission to issue a declaration to clarify the Recommendation under Article 8 (old Code) pertaining to collective groups.

The declaration to read as follows :—

> Certain biological groups which have been proposed distinctly as collective groups, not as systematic units, may be treated for convenience as if they were genera, but they require no type species. Any specific name proposed in combination with a collective group name shall have the same status as if it had been proposed in combination with a generic name of a systematic unit.

The declaration is twofold :

(1) To retain the collective groups as genera for convenience with the interpretations you have given in your letter to Dr. Walton. Except that, although not mentioned in the resolution, the same spelling as that of a collective group name should never be employed for a generic name of a systematic unit.

(2) That any specific name proposed for a new species in combination with a collective group name shall have the same status as if it had been proposed in combination with a generic name of a systematic unit.

ANNEXE 4 TO DOCUMENT 1/14

Copy of a letter dated 27th November 1956 from RAYMOND C. MOORE (*University of Kansas*) to FRANCIS HEMMING (*London*)

In view of the communications sent to you by Dr. A. C. Walton indicating that " collective group " nomenclature seems adequate for purposes of binominal nomenclature of life-stages such as those studied by parasitologists, it may be well to remove this from the application which we submitted on parataxa. Seemingly, an essential difference between collective groups and parataxa is that concept of type species, type specimens, and the like are not used with the former whereas they are essential in connection with parataxa.

ANNEXE 5 TO DOCUMENT 1/14

Copy of a letter dated 5th December 1956 from FRANCIS HEMMING (*London*) to RAYMOND C. MOORE (*University of Kansas*)

As regards the point raised in your letter, and my correspondence with Dr. A. C. Walton, it seems to me that the question of whether or not collective groups on parataxa should be accepted (a) as concepts not having type species or (b) as concepts having type species, is one of fundamental importance, for obviously if names given to such units are to be given type species as in the case of ordinary genera, the problem is one of considerably greater complexity than if they are not. In view of the differences of opinion which have been expressed on this question it will be particularly necessary that the Commission should be furnished with a representative sample of the view of specialists on this aspect of the matter before it takes a decision.

ANNEXE 6 TO DOCUMENT 1/14

Copy of a letter dated 8th January 1957 from RAYMOND C. MOORE (*University of Kansas*) to FRANCIS HEMMING (*London*)

After discussion with some parasitologists at the meeting of the Society of Systematic Zoology in New York City, I am not sure that all agree with the views expressed by Dr. Walton but so far as I am concerned reference to " life stages " in our application on parataxa could be deleted without affecting in any way the crucial objectives concerned with discrete parts of fossil organisms. However, I do think that the question of requirement of type species or lack of such requirement as affects names for " collective groups " is important. I am very sure that efforts to apply collective group nomenclature to our discrete fossils would not solve problems, mainly because we must have names both for the parts and for assemblages of parts presumed or demonstrated to represent complete animals.

ANNEXE 7 TO DOCUMENT 1/14

Copy of a letter dated 13th August 1956 from ALLEN McINTOSH (*Chairman, Committee on Nomenclature, American Society of Parasitologists*) to NORMAN D. STOLL (*Rockefeller Institute for Medical Research*) (transmitted by Allen McIntosh).

Herewith are copies of letters from the other members (Hunter, Kruidenier, and Wharton) of the A.S.P. Committee on Nomenclature and Terminology, of which I am Chairman. These pertain to the possibility of incorporating a new article in the Code for " parataxa " as recommended by Moore and Sylvester-Bradley, with supplementary suggestion by Commissioner Chester Bradley for broadening the plan to include " Collective groups " names as used by the helminthologists.

Granting that the " Parataxa plan " may be an excellent working tool for the paleontologists, such a plan, as outlined by Dr. Moore and Mr. Sylvester-Bradley, would, in the unanimous opinion of the A.S.P. Committee, handicap the work of helminthologists if adopted by them.

It is true that the category " Collective groups ", established for larval stages of worm parasites in which only a fragment in the life-cycle is known, is somewhat parallel to the proposed " parataxa ", the latter to include fossil fragments that are inadequate for identification. There is, however, a difference. In the case of the parasites we are working with living animals and have been able to resolve the complex life-cycle for many of the species placed in the " Collective groups ".

To accept and practice the concepts of " parataxa " as outlined by the two eminent paleontologists would be acting contrary to the Law of Priority, Article 25, and the Application of the Law of Priority, Article 27.

Some nomenclaturists may argue that to accept the Recommendation under Article 8, that certain biological groups which have been proposed distinctly as collective groups, not as systematic units, but may be treated for convenience as if they were genera, would be contradictory to the present rules. But such is not the case. Most helminthologists, in general, have not considered that the Recommendation under Art. 8 is inconsistent with or contrary to the rules and it has been their practice to accept as the valid name of a species only that name under which it was first designated on the condition : (a) That this name was published and accompanied by indication, or a definition, or a description ; and (b) That the author has applied the principles of binary nomenclature (Art. 25).

Even in the case of collective groups where a name, for convenience, is treated as a genus, the practice is not in violation of Article 27, for the Law of Priority obtains and consequently the oldest available name is retained: (a) When any part of an animal is named before the animal itself; (b) When any stage in the life history is named before the adult; (c) When the two sexes of an animal have been considered as distinct species or even as belonging to distinct genera; (d) When an animal represents a regular succession of dissimilar generations which have been considered as belonging to different species or even to different genera (Art. 27).

The practice of combining a new specific (trivial) name with a collective group name (a genus for convenience) does not create a scientific name contrary to binary nomenclature. (Art. 25b).

Many of the names of the " Collective groups " that are treated as genera for convenience, were proposed as genera but are synonyms of valid genera. If types were designated for other " Collective groups " and the name treated as generic taxa, they too would become synonyms of older genera. Some of these names were proposed for the purpose of serving as genera for larval stages of certain species that had similar characteristics. For such names (genera for convenience) no types were designated nor were any needed as the authors were aware that when the life histories were known several named genera would probably be represented under the collective group names. Other names that have come into use, as collective groups, were proposed as valid genera with types designated at the time or at a later date, for what were believed to be, in some cases, free-living animals but proved later to be a free living stage in the life cycle of parasites. Mueller's (1773) genus *Cercaria* included such forms.

It has been argued that specific names combined with collective group names (genera for convenience) are never combined with genera of systematic units. This is not the practice of helminthologists as the following examples will serve to indicate: 1. *Schistosomatium douthitti* (Cort, 1914) is the accepted name for the blood trematode of the muskrat and other rodents. This parasite was first known in the larval stage as *Cercaria douthitti* Cort, 1914. Before the life history of *Cercaria douthitti* was known this parasite was of considerable medical importance as a producer of schistosome dermatitis (" swimmer's itch "). Thus genus *Schistosomatium* was proposed by Tanabe in 1923 as a monotypical genus with *Schistosomatium pathlocopticum*, Tanabe, 1923, as type. Tanabe had first observed this parasite as a *furcocercous cercaria* in *Lymnaea palustris*. He successfully developed the adult of this blood flude parasite in mice.

In 1929 Helen F. Price, under the direction of Dr. George R. La Rue, University of Michigan, worked out the life history of *Cercaria douthitti* Cort, 1914, obtaining the adult in rats and mice. The adults occur as natural infections in *Ondatra zibethica* and *Microtus p. pennsylvanicus.* Miss Price transferred the specific name from the collective groups name, Cercaria (the genus *Cercaria* was proposed by Mueller, 1773, for a number of free swimming organisms that included among them some species of trematodes in their free swimming larval stage) to the genus *Schistosomatium* Tanabe, 1923, and today this parasite is known as *Schistosomatium douthitti* (Cort, 1914) Price, 1929, Syns. *Cercaria douthitti* Cort, 1914, and *Schistosomatium pathlocopticum* Tanabe, 1923.

2. Another interesting parasite, in which the name was first proposed in combination with a collective genus, is *Leucochloridiomorpha constantiae* (Mueller, 1935) Allison, 1943, a trematode in which the adult stage is found in a duck, *Anas rubripes.* This parasite was first known and described from an immature stage found in a snail, *Campeloma decisum* Say, and described under the name *Cercariaeum constantiae* by Mueller, 1935. The adult was described by Gowar, 1938, as *Leucochloridiomorpha macrocotyle* n.g., n.sp., Allison (1943) in publishing the results of his investigations on the life cycle of *Cercariaeum constantiae* found this larval species to be identical with Gower's *Leucochloridiomorpha macrocotyle.*

The accepted name today of this parasite is *Leucochloridiomorpha constantiae* (Mueller, 1935) Allison, 1943 ; Syns. *L. macrocotyle* Gower, 1938 ; and *Cercariaeum constantiae* Mueller, 1935.

3. Some additional collective group binominals in which the specific names have been combined with genera of systematic units :

(a) *Nematodium passali* Leidy, 1852 [=*Chondronema passali* (Leidy, 1852) Christie and Chitwood, 1931].

(b) *Cercaria variglandis* Miller and Northup, 1926 [=*Microbilharzia variglandis* (Miller and Northup, 1926) Stunkard, 1951].

(c) *Dubium erinacei* Rudolphi, 1819, Syn. *Sparganum erinacei-europaei* (Rud. 1819) Diesing, 1854 [=*Spirometra erinacei* (Rud. 1819) Mueller, 1937].

(d) *Cercaria elephantis* Cort, 1917, [=*Spirorchis elephantis* (Cort, 1917) Wall, 1941].

(e) *Diplostomulum joyeuxi* Hughes, 1929 [=*Szidatia joyeuxi* (Hughes, 1929) Dubois, 1938].

(f) *Aganofilaria georgiana* Stiles, 1906, [=*Filaria georgiana* (Stiles, 1906) Castellani and Chalmers, 1910].

Some of the " Collective groups " have had only a few specific names assigned to them and in the case of *Amphistomulum Brandes*, 1892, no species have been assigned.

It is my opinion that the helminthologists should either go on record as opposing the suppression of the Recommendation under Art. 8 of the Rules pertaining to collective groups, or, recommend to the International Commission on Zoological Nomenclature the rendering of a *Declaration* to read as follows :—

> Certain biological groups which have been proposed distinctly as collective groups, not as systematic units, may be treated for convenience as if they were genera, but they require no type species. Any specific name proposed in combination with a collective group name shall have the same status as if it had been proposed in combination with a generic name of a systematic unit.

ANNEXE 8 TO DOCUMENT 1/14

Copy of a letter dated 30th November 1956 from J. CHESTER BRADLEY (*Cornell University, Ithaca*) to A. C. WALTON (*American Society of Parasitologists*) (transmitted by Allen McIntosh).

I have been giving thought to your recent letter. Some questions arise.

(1) What provision should be made concerning homonymy ? Should names of collective groups enter into homonymy with one another ? With generic names ?

(2) The *Règles* provide, in effect (cf. *Bull. zool. Nomencl.* **4** : 226, par. 21) that a specific name, in order to become available, must be published in connection with a specified generic name. This seems to be a fundamental principle of zoological nomenclature. Would you care to propose an amendment to bring it into line with the Society's resolution ?

(3) The *Règles* provide (cf. *Bull. zool. Nomencl.* **4** : 114, par. 16) that the name of a hypothetical concept shall have no status. Since a " collective group " is not related to taxonomy, will it be argued by some that it is a hypothetical concept and therefore excluded from zoological nomenclature ?

(4) A taxon is any taxonomic unit—but this excludes a collective group. Without a type such a group cannot be objectively defined—but perhaps no definition is necessary. Should one conclude that being non-taxonomic it has no place in taxonomy, that it belongs to no family, order or class ?

In my proposed draft of the *Règles* I have frequently used the word taxon in wording a provision. Since all such provisions would exclude collective groups (as not coming under taxa) should not your resolution have some provision to the effect that wherever the word " taxon " is employed in the *Règles*, it should be understood that the provision in question likewise applies equally to a collective group ? Without reading through the entire draft I cannot tell whether this should not always be true, nor how important it would be.

(5) Subsequent to 1930 a generic name does not become available until published with a type-species. Since names of collective groups require no type species, should there not be some clarification of their status inserted in that provision ? I mean should one say "A generic name but not the name of a collective group " does not become available etc. ?

It would be helpful to know whether the Resolution enclosed in your letter was adopted (a) by mail vote of the membership of your society, or (b) by the nomenclature committee only. It would also be useful to know whether the vote, however taken, was unanimous, or if not what proportion was affirmative.

Personally, I feel that exceptions made for special disciplines are objectionable in the *Règles* and should be held down to a minimum. I had hoped that the proposal for parataxa might also cover the requirements of parasitologists, or be modified so as to be acceptable to them as well as to paleontologists. I am disappointed that your letter dismisses them without any explanation of why they will not serve.

ANNEXE 9 TO DOCUMENT 1/14

Copy of a letter dated 19th December 1956 from ALLEN McINTOSH (*Chairman, Committee on Nomenclature, American Society of Parasitologists*) to J. CHESTER BRADLEY (*Cornell University, Ithaca*) (transmitted by Allen McIntosh)

Your letter of November 30th 1956, to Dr. A. C. Walton, Secretary, American Society of Parasitology, Knox College, Galesburg, Illinois, has been referred to me for reply to the questions you have raised regarding the Resolution presented by the American Society of Parasitologists pertaining to collective groups.

These questions are answered in the order in which they appeared in your letter.

(1) Names of collective groups should be treated as if they were generic names, entering into homonymy with one another and with generic names. (Several names that are today used as collective groups were originally proposed as generic names.)

(2) Names of collective groups should not present an insurmountable problem. Why not state in Article 25, provoso (c), that such names are to be treated as if they were generic names ?

With reference to specific names to become available (cf. *Bull. zool. Nomencl.* **4** : 228, par. 21, last part) it is suggested that the underscored words be inserted in the following paragraph quoted from the above-mentioned reference :

The Commission agreed to recommend :—

> that words should be inserted in Article 25 to make it clear that the status of a trivial name (specific, subspecific or infra-subspecific) is not adversely affected where the generic name (including name of a collective group) with which it was combined when first published is a name which was itself either an unavailable name by reason of its having been published in conditions which do not satisfy the requirements of Article 25 (Law of Priority) (proviso (c) cases) or was invalid under the Law of Homonymy.

(3) Names of collective groups should not be treated as " hypothetical concepts " (cf. *Bull. zool. Nomencl.* **4** : 144, par. 16). They are names applied to certain immature forms, the categories of which are supra-generic in scope, and for convenience are treated as if they were genera.

Dr. Ch. Wardell Stiles (1905) in " The determination of generic types " stated his views concerning collective biological groups as follows : " Collective groups of this kind are of course unnatural, but they are nevertheless convenient, for they enable an international specific nomenclature for certain forms without recourse to classifying worms in an uncertain manner in genera which have a more or less definite status ". Dr. Stiles went on to say, " In case species are temporarily classified in such collective groups, we believe their specific names should be entitled to priority when they are definitely classified in their proper genera ". These quotations from Dr. Stiles's work are mentioned here since he has influenced the taxonomic and nomenclatural work of many helminthologists.

(4) Collective groups should not be classed as non-taxonomic units as all the species can be placed in classes, most of them in orders and many in families.

It would do no harm if our Resolution contains some provision to the effect that wherever the word " taxon " is employed at the generic level in the *Règles*, it should be understood that the provision in question applies equally to a collective group, except that a collective group does not require a type species.

(5) Since generic names, Article 25, proviso (c), are not available until published with a type species, there should be inserted in the proviso an exception for collective groups, since, as you have pointed out, they require no type species.

The Resolution was first approved by the nomenclature committee of the American Society of Parasitologists. It was then presented at the Council meeting of the Society at the annual meeting held at Storrs, Conn., August 26th, 1956. The Council, which consists of sixteen voting members of the Society, adopted the Resolutions unanimously. At the annual luncheon and business meeting of the Society, August 29th 1956, the action of the Council was approved unanimously by the members assembled, estimated at over two hundred present.

Your objections to exceptions made for special disciplines are understandable and appreciated. But, we believe our Resolution is more in the nature of a clarification (of the Recommendation under Article 8, old code) than a request for a special exception.

It is regretted that you were not informed as to the Nomenclature Committee's reaction regarding parataxa. One of our objections to parataxa, as outlined by Moore and Sylvester-Bradley, is that it violates the principle that a name applied to any part of any stage of an animal is to be considered in the same light as a name based upon any entire adult animal. In parataxa a wall would be conceived separating parataxa nomenclature from taxa nomenclature, disregarding the Law of Priority.

" Parataxa " may be an excellent plan for the palaeontologists, but in the field of parasitology it would not be practical as here we are working with living animals and have been able to resolve the complex life-cycles for many of the species that were once placed in " collective groups ", and hence to definitely place them in taxonomic genera.

I am enclosing a copy of a letter to Dr. Norman R. Stoll (August 13th 1956) containing additional background data.

DOCUMENT 1/15

Arrangements made between the Office of the Commission and Professor Raymond C. Moore for making an organised attempt to obtain comments on the "Parataxa Plan" from representative palaeontological institutions and individual specialists.

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Commission Reference : Z.N.(S.) 1056)

In 1956 correspondence took place between the Office of the Commission and Professor Raymond C. Moore on the question of the need for taking special measures to obtain comments on the "Parataxa Plan" submitted by Mr. P. C. Sylvester-Bradley and himself from representative palaeontologists in different parts of the world. This led to Professor Moore kindly undertaking to prepare for this purpose a succinct digest of the principal proposals embodied in the foregoing Plan.

2. The document so prepared by Professor Moore was despatched by the Office of the Commission on 8th July 1957 to a large number of palaeontological institutions and individual palaeontologists. In addition, copies of Professor Moore's digest were sent to a number of other zoological institutions and specialists whose views were sought on the question of possible repercussions on the current system of nomenclature for species (i.e. of the nomenclature of "whole-animals" in the terminology of the Moore/Sylvester-Bradley Plan) of the introduction into the *Règles* of provisions for the naming of discrete parts of animals unidentifiable as whole-animals, such parts to be reorganised under the title "parataxa".

3. The Letter so issued, together with the digest prepared by Professor Moore, is reproduced in the Annexe attached to the present note.

ANNEXE TO DOCUMENT 1/15

Letter dated 8th July 1957 issued by the Office of the Commission to certain palaeontological and other zoological institutions and to certain specialists inviting comments on the "Parataxa Plan".

Proposed insertion in the "Règles" of a provision defining, and providing for the nomenclature of "Parataxa" (=discrete parts of animals unidentifiable as belonging to whole-animal genera and species).

Among the papers which will be considered by the Fifteenth International Congress of Zoology, London, 1958, will be three papers on the above subject submitted jointly by Professor Raymond C. Moore (*University of Kansas*) and Mr. P. C. Sylvester-Bradley (*University of Sheffield*). These papers will shortly be published in the London Congress Agenda Volume (Volume 15) of the *Bulletin of Zoological Nomenclature.*

It has been agreed between Professor Moore and this Office that in view of the novelty and complexity of the issues raised by the foregoing proposals it is desirable that exceptional measures should be taken before the Congress to ascertain the views of specialists on the action recommended. For this purpose Professor Moore has prepared a digest of the proposals in question which it has been agreed should be submitted to a number of specialist bodies and individual specialists for observations. A copy of Professor Moore's digest is enclosed herewith and it is hoped very much that you will be so good as to furnish observations to this Office on the action recommended.

Two groups of issues are involved, namely :—(1) Are the proposals appropriate for the palaeontological purposes which they have been devised to serve ? (2) Are the safeguards proposed adequate to render the introduction of the scheme innocuous from the point of view of the nomenclature of species and subspecies (whole-animal nomenclature) ? The first of these questions is primarily of interest to palaeontologists ; the second is of importance to all zoologists, neontologists as well as palaeontologists.

Enclosure to letter issued by the Office of the Commission reproduced above as the Annexe to Document 1/15

Proposed addition to the " Règles " of provisions recognising and regulating the nomenclature of " parataxa "

Plan submitted jointly by RAYMOND C. MOORE (*University of Kansas, Lawrence, U.S.A.*) and P. C. SYLVESTER-BRADLEY (*University of Sheffield, England*).

Request to specialist bodies and specialists for advice.

(Note by the Secretary to the International Commission on Zoological Nomenclature.)

Arrangements have been made by the International Trust for Zoological Nomenclature for the publication at a very early date in the special London Congress (1958) Agenda Volume of the *Bulletin of Zoological Nomenclature* (Volume 15, Part 1/4) of a group of three papers written jointly by Professor Raymond C. Moore (*University of Kansas, Lawrence, Kansas, U.S.A.*) and P. C. Sylvester-Bradley (*University of Sheffield, England*) containing proposals for the incorporation in the International Code of Zoological Nomenclature of a group of provisions recognising and defining the concept " parataxon " and providing for the regulation of the nomenclature of units belonging to this category.

2. In view of the novelty of the proposed plan and the inevitable complexities involved it is evident that the Fifteenth International Congress of Zoology when it meets in London in 1958 is likely to wish to satisfy itself not only that the proposals are well-conceived but also that they command a representative body of support among palaeontologists. This is all the more important in the present case in view of the fact that its subject is primarily of interest to palaeontologists, a body of specialists who are normally not strongly represented at International Congresses of Zoology. In these circumstances consideration has been given by Professor Moore and myself to the question of the procedure to be adopted for bringing the foregoing proposals prominently to the attention of interested specialists in advance of the London Congress.

3. It is to be hoped that in part the foregoing object will be achieved by the publication in the *Bulletin of Zoological Nomenclature* of the detailed papers prepared by the applicants and also by the issue of Public Notices to representative serial publications at the time when these applications are published in the *Bulletin*. Experience has shown however that in the case of exceptionally complicated problems and also in that of cases of a general—as contrasted with a specialised—interest the foregoing procedure is not always capable of securing as fully a representative sample of comments as is desired. This problem arose in 1952 in connection with proposals affecting certain broad issues touching the provisions of the *Règles*. It was then decided that the normal methods for obtaining the views of interested specialists should be supplemented by the distribution of questionnaires to specialist bodies and individual specialists. The replies to these questionnaires were published in the special Copenhagen Congress (1953) Agenda volumes of the *Bulletin* (Volumes 8 and 10) immediately upon their receipt in the Office of the Commission. It has been decided to adopt a similar procedure in the present case.

4. In pursuance of the foregoing decision Professor Moore has kindly prepared a digest of the lengthy documents in which he and Mr. Sylvester-Bradley had discussed the problem of the nomenclature of parataxa, in which he drew attention to the principal issues involved and gave particulars of the provision which it was recommended should be inserted in the *Règles*. The digest so prepared by Professor Moore is attached to the present note as an appendix.

5. It is particularly hoped that the specialist bodies and individual specialist to whom the present document is being despatched will be so good as to assist the International Commission on Zoological Nomenclature and, through it, the Fifteenth International Congress of Zoology, London, 1958, by communicating statements to this Office :—

(1) setting out their views on the proposals submitted by Professor Moore and Mr. Sylvester-Bradley ;

(2) indicating, if they consider that those proposals ought to be expanded or otherwise amended in any respect, how the changes desired should be fitted into the general framework of the plan submitted in this case.

6. Answers to the present appeal for advice should be addressed to the Secretary, International Commission on Zoological Nomenclature (address : 28 Park Village East, Regent's Park, London, N.W.1). Communications so received will be published in the Agenda volume of the *Bulletin of Zoological Nomenclature* immediately upon their being received. In view of the importance attached to the early publication in the above volume of the *Bulletin* of comments on all matters of nomenclature to be brought before the London Congress next year, it will be particularly appreciated if recipients of the present request for assistance will be so kind as to furnish replies at the earliest date which they may find to be practicable.

(signed) FRANCIS HEMMING
Secretary to the International Commission
on Zoological Nomenclature

4th July 1957.

Appendix to the Note by the Secretary dated 4th July, 1957

Digest of an application submitted by R. C. Moore and P. C. Sylvester-Bradley for the introduction into the " Règles " of provisions recognising parataxa as constituting a special category for the classification and naming of Discrete Parts or Life-Stages of Animals which are inadequate for identification as Whole-Animal Taxa.

Digest prepared by **RAYMOND C. MOORE**
(*University of Kansas, Lawrence, Kansas, U.S.A.*)

The purpose of applications proposing recognition of classifactory units termed " parataxa " is to remove instability and confusion affecting the nomenclature of several thousand kinds of already named Discrete Parts of animals (almost exclusively fossils) which are unidentifiable as belonging to generic and specific taxa of Whole Animals or are even unplaceable in supra-generic taxa. A large majority of these fragmentary remains have been demonstrated to possess high value in stratigraphic paleontology. Similarly, certain life-stages of parasites, very important in medical studies, are indeterminable as to the taxa of adults and possibly those may be usefully defined as parataxa.

2. As a basic premise we may agree that zoological taxonomy comprises a single system based on presumed natural relationships and into this system all animals, with varying degree of success, may be fitted. However, because many Discrete Parts of animals cannot be incorporated in any Whole-Animal taxa, a system of parataxonomy that is measurably independent of zoological taxonomy is called for. Then classification and nomenclature applicable to the Discrete Parts of unidentified animals may proceed without confusion or disturbance of either category (taxa or parataxa) with the other. It is needful that all rules governing nomenclature of whole-animal taxa should be applied without any difference in mode or force to the category of parataxa as used for Discrete Parts of unidentifiable Whole Animals, except that for purposes of the Law of Priority a wall should be conceived to separate nomenclature of Whole-Animal taxa from nomenclature of parts defined as parataxa. For the purposes of the Law of Homonymy, taxa and parataxa would be co-ordinate.

3. Names given to Discrete Animal parts representing unknown Whole-Animal taxa have been treated in three different ways, none of them satisfactory. All consists of binomina for " specific " units and trinomina for " subspecific " units.

(a) Names construed to differ in no way from those employed for Whole-Animal taxa and conforming to Article 27 (a) of the *Règles* which stipulates that " the oldest available name is retained when any part of the animal is named before the animal itself " ;

(b) Names construed as " technical terms " (Paris, 1948) which are rejected from the domain of zoological nomenclature ; and

(c) Names construed to designate " form " taxa analogous to " form-genera ", " organ-genera ", " form-species ", etc., of the Botanical Code but not recognised by zoological rules.

4. If nomenclature of Discrete Animal parts is governed exactly in the manner accepted for naming of Whole Animals (see paragraph 3(a) above), there would be a continuous risk of confusion and instability of nomenclature which may be serious. Out of many examples, two are cited briefly here.

(a) *Trigonellites lamellosus* Parkinson, 1811, based on an ammonoid aptychus (operculum) is the type species of *Trigonellites* Parkinson, 1811. It was described from a specimen not associated with a conch. Subsequently, *T. lamellosus* has been found *in situ* within conchs identified as *Oppelia flexuosa* (von Buch, 1831), *O. discus* (Quenstedt, 1856); and *O. euglypta* (Oppel, 1863). The genus *Oppelia* Waagen, 1869, is based on *Ammonites subradiatus* Sowerby (J. de C.), 1823, as type species, described from a conch. If *Trigonellites* and *Oppelia* are correlative competing names, Article 27 (a) would call for the acceptance of *Trigonellites* and the rejection of *Oppelia*, despite very long and widespread use of *Oppelia* for scores of species, including fossil zone-guide forms. Also, it would be inadmissible for three valid species as defined by conchs (*O. flexuosa*; *O. discus*; and *O. euglypta*) to be synonymized under the name *Trigonellites lamellosus*.[1 and 2]

(b) A Whole-Animal (?) species named *Scottognathus typicus* (Rhodes, 1952) is based on 132 natural assemblages of Pennsylvanian (Upper Carboniferous) conodonts. *Scottognathus* Rhodes, 1953, is a substitute name for *Scottella* Rhodes, 1952 (*nec* Enderlein, 1910). Discrete components of these assemblages are conodonts named *Hindeodella delicatula* Stauffer & Plummer, 1932; *Synprioniodina microdenta* Ellison, 1941; *Idiognathodus claviformis* Gunnell, 1931; *Streptognathodus excelsus* Stauffer & Plummer, 1932; and *Ozarkodina delicatula* Stauffer & Plummer, 1932. Generic synonymy of *Scottognathus* and of one or more of its constituents can be established only if the type species of discrete conodont genera recognized in the assemblages is present. *Hindeodella* and *Synprioniodina*, Bassler, 1925 and *Ozarkodina* Branson & Mehl, 1933, are not represented by their type species, whereas *Idiognathodus* Gunnell, 1931, and *Streptognathodus* Stauffer and Plummer, 1932 are represented by their type species. Accordingly, under Article 27, *Scottognathus typicus* must yield to *Idiognathodus claviformis* as the name for the assemblage. Out of sixty-four nominal species of *Idiognathodus* and forty-three of *Streptognathodus*, all but thirty-one are listed as synonyms of discrete conodonts found in such association with "*Scottognathus*" *typicus* as to indicate that they have been derived

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1. It should be noted here that in 1954 Dr. W. J. Arkell made an application to the International Commission for the grant of a *Declaration* excluding from availability for the purposes of zoological nomenclature, i.e. for the purposes of what Professor Moore here terms the nomenclature of "whole-animal taxa" of any name based solely upon the aptychus of an ammonite and proposing the addition to the *Official Indexes of Rejected and Invalid Names* of a number of such names, including the name *Trigonellites* Parkinson, 1811, here cited by Professor Moore as an example (Arkell (W.J.), *Bull. zool. Nomencl.* 9 : 266–269). No action has as yet been taken by the International Commission on the above application, it having been considered better to defer action thereon until a decision had been taken by the International Congress of Zoology on the wider issues of a general character which it was known that Professor Moore and Mr. P. C. Sylvester-Bradley proposed to raise, i.e. the proposals put forward in the papers, of which Professor Moore has here given a digest. (initialled F. H. 4th July 1957.)

2. See Document 1/4 on pages 71–75 of the present Case.

from animals conspecific with this species. It is difficult indeed to see by what names these forms should be described under the present *Règles.*

5. Names treated as " technical terms " which are expressly rejected as zoological names lack government by the Laws of Homonymy and Priority, as well as other regulation, thus promoting chaos in nomenclature. Virtually all names published for parts of unidentified whole animals are binomina composed of " generic " and " specific " Latinized names exactly similar to zoological names and because most fossils are varyingly incomplete, discrimination between those considered suitable for nomenclature under zoological rules and those excluded from such treatment is wholly subjective. Indeed, the probably complete skeletal remains of some organisms assignable to protistan groups have been named by Deflandre using intended " Technical term " procedure whereas most authors would consider the published names (without regard to intent of the author) as undeniably acceptable zoological names. Here lies confusion.

6. The concepts of " form-genera " and " form-species " might be adapted advantageously to classification and nomenclature of Discrete Parts of unidentified animals but they contravene the rules of zoological nomenclature. It seems preferable to recognise a special category of " associate taxa ", that is, parataxa, which would be correlative with all zoological names for the purposes of the Law of Homonymy but would constitute a wholly segregated group for purposes of the Law of Priority. In this way instability and confusion of nomenclature would be easily avoided. In order to avoid subjective variation, in deciding what zoological objects are to be classified and named in terms of parataxa, rather than in those of whole-animal taxa, it is recommended that this be determined solely by the Commission.

7. Recommendation is made accordingly :

(i) that Article 27 (a) should be modified by adding the phrase " except for parataxa ", thus excluding the classificatory units called parataxa from application of the Article : and

(ii) that a new Article should be incorporated in the *Règles* :—

(a) defining " parataxon " as a taxonomic category comprising Discrete Parts or Life-Stages of animals, which, by decision of the Commission, are deemed to be unidentifiable in terms of the whole animals that produced them ;

(b) stipulating that classification and nomenclature of any group of Discrete Parts or Life-Stages of animals in terms of parataxa shall be allowed only after the Commission has ruled to this effect and then such ruling shall apply retroactively irrespective of whether an author uses the term " parataxa " ; and

(c) providing that the nomenclature applied to taxa and parataxa shall be mutually exclusive and independent for the purposes of the Law of Priority, but co-ordinate for the purposes of the Law of Homonymy, names belonging to one category not being transferable to the other.

DOCUMENT 1/16

Parataxa nomenclature in relation to whole-animal nomenclature

Correspondence between Francis Hemming, Raymond C. Moore and P. C. Sylvester-Bradley

(Commission Reference : Z.N.(S.) 1056)

Editorial Note : The Document Number 1/16 has been allotted to letters and other papers on the subject specifically of the possible repercussions of the recognition of parataxa nomenclature on whole-animal nomenclature.

ANNEXE 1 TO DOCUMENT 1/16

Copy of a letter dated 5th July 1957 from FRANCIS HEMMING (*London*) to RAYMOND C. MOORE (*University of Kansas*) and P. C. SYLVESTER-BRADLEY (*University of Sheffield*).

I am sure, if I may say so, that you and Mr. Sylvester-Bradley were right when you decided to base your plan for the recognition of parataxa on the basis that the nomenclature of such units should be independent of, and should not interfere with, zoological nomenclature as currently understood, that is what you call " whole-animal nomenclature ". It seems to me, however, that in one or two respects your scheme needs further consideration from this point of view.

2. The problems with which we are here concerned are very similar to those which faced the International Congress of Zoology in Paris in 1948 when it considered the question of granting some kind of recognition in the *Règles* to names in what it was then decided to call " infra-subspecific forms ". As you no doubt know, there are some branches of whole-animal nomenclature the literature of which is weighed down by innumerable names given to individual aberrations, i.e. to individual specimens differing in some respect from what the

authors giving the names consider to be the " normal " form of the population in question. At that time the *Règles* contained no clear provision on this matter, and in the particular groups concerned a growing body of workers attached importance to the naming of these individual infra-subspecific forms, while a much larger number of zoologists, while disliking the naming of such forms, felt bound to take account for the purposes of the Law of Homonymy of the names published for such forms. It was to resolve this dilemma that the Paris Congress decided that, while names given to infra-subspecific forms should be recognised, such names should be treated as belonging to a system independent of that prescribed for species and subspecies, i.e. for populations. This basic feature of the Paris decision on infra-subspecific forms has been copied in your plan for the recognition of names for parataxa, but I feel that in its present form your plan does not provide as completely as is required for the independence of parataxa nomenclature and whole-animal nomenclature.

3. When the parallel problem was being considered in Paris, it was found necessary to give consideration to the form in which names had been published for taxa currently regarded as applying not to populations (species or subspecies) but to aberrations and other minority forms. It seems to me a somewhat similar problem arises in the present case. The scheme will require to be such as to apply appropriately to names published in any of the following ways :—

(1) After the acceptance of your plan and its coming into operation, it may certainly be expected that palaeontologists will start publishing papers containing new names which they will expressly state are names given to parataxa and not to whole-animal taxa.

(2) Names already published for " form genera " and the like.

(3) Names already given expressly to discrete parts of fossils and published as being names belonging to the categories *Legio*, *Cohors*, *Manipulus* or *Centuria* under the scheme devised by M. Georges Deflandre, or under similar schemes devised by other specialists if such schemes exist.

(4) Names stated by their authors as being based upon some object which is, (a) expressly stated to be, or (b) is later determined by specialists to be, a discrete part of some whole animal, for example a name either expressly stated by the author to be based upon the aptychus of an ammonite or later determined as having been so based.

4. In its present form your scheme does not seem to me to deal satisfactorily, or at least not as satisfactorily as it could, with each of the classes

of names enumerated above. First, names belonging to class (1) above should I suggest be brought within the parataxa scheme directly in the new Article, it being quite unnecessary that names of this group should be subject to prior reference to the Commission. Similarly, I consider that names belonging to classes (2) and (3) above, i.e. names expressly given for form genera, etc., and names expressly given as belonging to the special *Extra-Règles* categories devised by M. Deflandre should also be brought directly into the scheme and should not be made subject to reference to the Commission.

5. We now come to class (4) above. For the purposes of discussion it will I think be convenient to take as an example names based upon the *Aptychi* of Ammonites. Under your scheme (point (2)(b)), it would be open to any zoologist to submit an application to the Commission for a Ruling that a name based upon the aptychus of an ammonite is to be treated as a name belonging to the parataxa system, and not to the whole-animal system of nomenclature. Once such a Ruling had been given, the position would be perfectly clear in the case of any name, the author of which had stated expressly that he based the unit so-named upon an aptychus, for in that case a definite and objective criterion would be available for deciding to which of the systems of nomenclature (parataxa or whole animal) the name concerned belonged. It appears to me, however, that this would not be the case where an author described what he considered to be what you would call a whole-animal species but which some later author or authors considered was based upon an aptychus or on some other discrete part of a whole animal, for in that case only a subjective taxonomic judgement would be available for determining to which of the two systems of nomenclature the name should be considered to belong.

6. The ambiguity discussed above is one which will, I think, need to be solved. As a palaeontologist, you may say that from your point of view a subjective judgment of the foregoing kind is quite good enough, and no doubt for the purposes of parataxa taxonomy this would be true, but we have to look on this matter from the point of view of the whole-animal zoologists as well as of that of the palaeontologist. From the point of view of the former, a subjective taxonomic view by palaeontologists that a particular name was a name which should be regarded as that of a parataxon under a Ruling given by the Commission under point (2)(b) of your scheme, would not provide a satisfactory solution. What the whole-animal zoologist requires is some provision of a clear cut objective kind which would enable him to know whether the name in question belonged to the parataxa system and, therefore, would not interfere with whole-animal nomenclature, this being necessary if parataxa nomenclature and whole-animal nomenclature are to be genuinely independent of one another.

7. This brings me to your point (2)(c), the meaning of which I do not find to be clear. In this section you say that the names for whole-animal taxa and

for parataxa are to be mutually exclusive and independent for the purposes of the Law of Priority " but co-ordinate for the purposes of the Law of Homonymy ". The only point of the Law of Homonymy is to rule out as unavailable the later of any two homonyms. Accordingly if names for parataxa and names for whole-animal taxa are to be independent for the purposes of the Law of Priority, I cannot see how the Law of Homonymy could play any useful, or indeed any, part at all. My feeling is that as in the system for naming infra-subspecific forms so also in that for the naming of parataxa, the mutual independence should be complete and should apply therefore for the purposes of the Law of Homonymy as well as for those of the Law of Priority.

8. There is also a question of drafting in connection with your point (2)(b) which I should like to raise. As at present drafted, the only person who is entitled to ask the Commission for a Ruling is the zoologist who wants to have the classification and nomenclature of a particular group of animal fragments treated in terms of parataxa. I quite see that it is natural that you should have conceived of the scheme from this point of view, but it is necessary also that it should be looked upon from the standpoint of a zoologist who wants to get out of the way names based upon such units. For example, Dr. W. J. Arkell, when he made his application to the Commission (1954, *Bull. zool. Nomencl.* **9** : 266–269) for the suppression of the name *Trigonellites* Parkinson, 1811, and other names based on ammonite aptychi, was not in the least interested in the question of the use of names for discrete parts of ammonites, his sole object being to get such names out of the way and to prevent them from interfering with the ordinary nomenclature for ammonites. I think it clear that the wording of your point (2)(b) will need to be revised to take account of the foregoing considerations.

9. To sum up, it seems to me that :

(a) If there is to be a special world of parataxa nomenclature, there are certain classes of names which ought to be put into that world direct in the Article itself without the necessity of prior reference to the Commission ;

(b) Further consideration is, I think, necessary in regard to the status to be accorded to names published as names for whole-animal taxa but considered subjectively on taxonomic grounds by later authors as being names based on discrete parts of some whole animal ;

(c) For the reasons which I have explained, I feel that a name applicable to a parataxon should be independent of any name given to a whole animal not only as proposed in your paper for the purposes of the Law of Priority but also for those of the Law of Homonymy ;

(d) The wording of point (2)(b) in the plan requires further consideration so as to provide an approach to the Commission not only to zoologists who desire to see certain categories of names recognised as names for parataxa but also to zoologists whose sole aim is to eliminate certain such names from consideration for the purposes of whole-animal nomenclature.

10. I look forward to receiving your comments and suggestions on the above points at a very early date.

ANNEXE 2 TO DOCUMENT 1/16

Copy of a letter dated 9th July 1957 from P. C. SYLVESTER-BRADLEY (*University of Sheffield*) *to* FRANCIS HEMMING (*London*)

Thank you for your various letters on the " Parataxa Plan ", and for the careful reading you have given to the scheme, and for the comments and suggestions which, as a result, you have incorporated in your letter to Professor Moore of 5th July. I will in this letter set out my observations on these suggestions of yours.

In paragraph 3 of your letter you suggest that it will be necessary for the scheme to be applicable to names published in four different ways. I agree that this is so, and it will be convenient if I deal with each in turn. In passing, I should mention that I do not altogether agree with the suggestion you make in the same paragraph, that the regulations introduced to cater for infra-subspecific forms are in fact coping with a somewhat similar situation,as those that are now proposed for dealing with dual nomenclature. The two situations differ in many important respects, and any assumption that the problems of the two can be solved by similar regulations would seem to me dangerous. In particular I refer near the end of this letter to your observations on the Law of Homonymy. Finally, I will deal with your remarks on the drafting of our Proposal 2 (b).

1. *Regulations governing names given in the future to taxa expressly stated to be parataxa.*

The regulations framed by us in proposal 2 (a) are intended to ensure that this system of nomenclature will be applied only to groups of animals expressly listed by the Commission as available only for the creation of parataxa. In your letter you say that you consider names in any animal group expressly stated by the author in question to be those of parataxa should be accepted as such without prior reference to the Commission. The limitation we have suggested was introduced to avoid strong criticism from the many palaeontologists who would abhor the creation of parataxa in particular groups in which they are specialists. Accordingly we have attempted to set out reasons for seeking prior authorization from the Commission in paragraph 12 of our application, and have also referred to them in the second half of paragraph 8. Perhaps I can state the situation most clearly by citing examples : Discrete animal parts vary in value as indicators of whole-animal relationship. In our opinion, discrete conodonts and ammonoid aptychi possess insufficient diagnostic characters to allow recognition of whole-animal taxa. In this context, " our opinion " is of course subjective. In other groups of animals the degree of subjectivity varies widely. Some vertebrate palaeontologists might believe that fish-teeth formed a suitable group of discrete animal parts for " parataxonomy " ; I fancy, however, that most vertebrate palaeontologists would be opposed to this view on two grounds : (a) because they believe that many fish teeth possess sufficient diagnostic characters for the recognition of at least some degree of whole-animal taxonomy ; and (b) because the existence of a dual system of nomenclature such as would be brought about by the creation of such parataxa would bring confusion into a system where confusion does not exist at the moment. They might argue that no convincing case has yet been put forward showing that any advantage attaches to the creation of a dual system of nomenclature in this case and that the action of Art. 27 is satisfactory. Even stronger arguments have been sent to Moore and myself by vertebrate palaeontologists who have said that they would object to the recognition of parataxa consisting of discrete vertebrate bones of any kind.

Other examples are more controversial, which usually means they are more involved. In brief, I would say that my own feeling is that parataxonomy is not likely to be justifiable in any animal group in which unofficial dual systems of nomenclature do not already exist. One of the controversial cases involves dual systems which have been proposed for the discrete skeletal parts of crinoids, and in particular the discrete ossicles of crinoid stems. It is controversial (as we discovered by correspondence) because some specialists believe that the recognition of parataxa would introduce undesirable complications since some crinoid ossicles are sufficient for the diagnosis of whole-animal taxa. Less controversial cases concern dual nomenclatures already existing which deal with holothurian spicules and scolecondonts. We might, in fact, have included these groups as supplemental applications additional to those dealing with conodonts and aptychi if we had not felt that such recommendations would have necessitated adding both to the length and complexity of a subject which is already clearly long and complex enough.

Summarizing, the decision of whether an animal group is suitable for the creation of parataxa or not is a subjective one. Many of our correspondents believe that it would be a mistake to leave such subjective judgments to individual taxonomists, and argue that such a method would in many groups of animals introduce confusion into a situation which is at present adequately controlled by the provisions of Article 27(a). The only alternative is to give to the Commission the onus of deciding whether or not a particular animal group can usefully be classified into parataxa.

I should emphasize that once a group has been recognized by the Commission for this purpose, new names introduced for parataxa will automatically be valid without further reference to the Commission. Thus if the Commission were to adopt our proposals regarding conodonts and ammonoid aptychi, new names introduced within these groups would automatically be regarded as those of parataxa.

2. *Names already published for "form-genera"*

These would automatically be regarded as parataxa providing the original authors had recognized them as names given to the discrete parts of groups of animals listed by the Commission under regulations concerning parataxa.

3. *Names given in schemes such as those devised by Deflandre and his wife*

No proposals have yet been made for the inclusion of the various protozoa studied by Deflandre, or the holothurian spicules studied by his wife, in the scheme of parataxa we present. In my opinion such proposals should be made, however, if the scheme is approved by the Commission.

4a. *Names expressly stated by their authors to be for discrete parts*

If such discrete parts are included in the Commission's list of those in which parataxa are to be recognized, the names will automatically be regarded as those of parataxa. If the discrete parts are not in the Commission's list, names applied to them will remain subject to the usual provisions of Article 27 until such time as they may be added to the list.

4b. *Names not expressly stated by the original author to be those of discrete parts, but subsequently and subjectively so determined by later authors.*

The subjective nature of arguments deciding whether such names should be applied to whole-animal taxa or parataxa was recognized by Moore and myself when we drew up our application, and we have considered it in some detail in our supplemental applications (e.g. para. 4(1) of that dealing with aptychi). I agree with you, however, that it would be wise to deal with this question in the general application, and enclose a draft of a possible additional paragraph (to follow on, I suggest, after para. 12) for the consideration of Professor Moore and yourself.

The Law of Homonymy as it affects parataxa.

I now come to the question of the Law of Homonymy. Your first comment concerning our para. 2(c) is that you do not find its meaning clear. I have re-read the paragraph carefully, and I cannot see where its meaning can be in doubt. It is customary for Plenary Powers to be used to suppress names for the purposes of the Law of Priority but not for those of the Law of Homonymy. The generic name *Striaptychus* Trauth, 1927 is a junior subjective synonym of *Acanthoscaphites* Nowak, 1911. If the names are regarded as mutually exclusive for the purposes of the Law of Priority, both can be used in their respective roles as the names of a parataxon and a whole-animal taxon respectively. On the other hand the name *Sidetes* Giebel, 1847, is that of an aptychus, and is therefore a name of a parataxon. According to our proposal, it would invalidate for the purposes of the Law of Homonymy any subsequent use of the name *Sidetes* either for a whole-animal taxon or for any other parataxon of generic rank. The regulation seems to me clear.

A second point with regard to the Law of Homonymy is the fact that I understand you disagree with the purpose of our proposed regulation, in that you believe there would be no disadvantage in allowing the co-existence of two valid generic homonyms such as *Sidetes* Giebel, 1847 (an aptychus) and *Sidetes*, say, Smith, 1957 (a whole-animal genus). There is, of course, room here for a difference of opinion. To my mind, to make such homonymy legitimate would be to invite confusion, a confusion which would surely be disastrous if the two homonyms happened to fall into closely related groups—for example, into ammonoid conchs and ammonoid aptychi respectively—or into conodont assemblages and into discrete conodonts.

The wording of our proposal 2 (b)

I see the force of the point you raise, but find it a little difficult to cater for ; I cannot, in fact, devise a satisfactory solution. The Commission can only judge on the advisability of admitting a specified group of animal parts to those in which parataxa are allowed if full details of the circumstances are presented to them. Such details can only be gathered by a taxonomist prepared to devote some time and trouble to the task. The preparation of the two supplemental applications on aptychi and conodonts which were undertaken by

Moore and myself were not written without a deal of preliminary research—greater, I suspect, than Arkell would have been prepared to spend on the question of aptychi. If the sole object of an applicant is to get rid of unwanted names, perhaps his best expedient is to do what Arkell did, and apply for the use of the Plenary Powers to suppress such names. If this were found to be objectionable by taxonomists using the names in question, they could be asked to prepare the necessary application for the recognition of parataxa in their group.

Summary

The substance of this letter may be considered under three headings.

(1) The arguments which led us to restrict the operation of regulations governing parataxonomy to animal groups expressly listed by the Commission are set out. These arguments are not necessarily decisive. There seems to me some force in your suggestion that any taxonomist should be able to adopt parataxonomy in his group without prior reference to the Commission provided his action affected only new names proposed by himself. Any such subjective and individual opinions should not, in my view, be allowed to change retroactively the status of names already validly proposed as those of whole-animal taxa. Such a proposal for a change or addition to the regulations proposed by us would seem to me best framed as a separate proposal.

(2) The suggestion that names given to parataxa should not be co-ordinate with those given to whole-animal taxa for the purpose of the Law of Homonymy runs counter to one of the main objects of our proposal. I would myself strongly oppose such a suggestion.

(3) I agree with your suggestion that our application needs an additional paragraph setting out the procedure desired to secure objectivity in deciding whether a name is that of a parataxon or not.

Draft of additional paragraph to R. C. Moore and P. C. Sylvester-Bradley's "Application for a Declaration recognising Parataxa"

13. Procedure to ensure objectivity in deciding whether a name is that of a parataxon or not.

The scheme here put forward can only function without ambiguity if an objective decision can be made as to whether a name is that of parataxon or not. To ensure this, it will be necessary for the Commission to lay down in clear

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[1] The figure '13' here cited by Mr. Sylvester-Bradley refers to the paragraph so numbered in the paper reproduced as Document 1/1 on the Agenda Paper (see page 12 of the present volume).

terms the form of evidence necessary to make such a decision. In the cases of the two groups of animals which we propose, in the supplemental applications that follow, as suitable for the recognition of parataxa, we have attempted to word the proposals with the requisite clarity. We suggest that a name can only be recognised as that of a parataxon in these groups if the original author makes it clear that the name in question is for a taxon consisting of respectively either the aptychi of ammonoids, or conodonts not regarded as natural assemblages.

Some names introduced, for example, for aptychi have only subjectively been so classed by later authors, the original author not having used the word " aptychus ". We suggest that these should not automatically be regarded as the names of parataxa, as subjective decisions by later authors are not valid for this purpose.

Such names, we suggest, should only be admitted as those of parataxa by operation of the Commission's Plenary Powers.

ANNEXE 3 TO DOCUMENT 1/16

Copy of a letter dated 13th July 1957 from RAYMOND C. MOORE (*University of Kansas*) to FRANCIS HEMMING (*London*)

I believe that I understand the parallel which you draw between parataxa and consideration of " infra-subspecific forms " as considered by the Congress in Paris in 1948. Possibly it is true that if parataxa nomenclature is to be set sharply apart from whole-animal nomenclature, your conclusion that the Law of Homonymy is no more significant than the Law of Priority as regards conflicts between names of the two groups seems logical. However, unless proposals for recognition of parataxa as a separate system of nomenclature are seriously diminished by letting the recommendations made by Sylvester-Bradley and me stand, I very much favor legislation that prohibits homonymy even in separate systems of nomenclature. I would go so far as to say it is lamentable that some names for plants are duplicates of those allowed for animals (1) because the supply of scientific names is not limited and (2) the boundary between plants and animals is a decidedly fluctuating one, without going into the question of Protista. In sum, I answer the points you raise about homonymy by saying that in spite of favoring strongly the recommendation submitted by Sylvester-Bradley and me, if the change in this regard makes for simplification of the whole proposal, thereby winning greater likelihood of its acceptance, I should regard this change as a strategic retreat.

Concerning the four categories of names listed in paragraph 3 of your letter, I readily agree with your opinion concerning numbers 1, 2, and 3. The fourth category of names is quite another matter, bringing up problems to which Sylvester-Bradley and I gave a great deal of thought, ending in judgment that the only way in which subjectivity of the individual could be avoided was by reference to the Commission. In this area we encounter the vexatious question of " Problematica ", but without mentioning these I turn to the point you raise expressing the viewpoint of the whole-animal neozoologists (or paleozoologists) who demand an objective criterion for classifying entities belonging to one or the other systems of nomenclature. I fear that a wholly objective basis for deciding this does not exist, even though in several large areas an easy differentiation can be made. There are innumerable isolated fragments of vertebrate skeletons (teeth, scutes or scales, otoliths, etc.) that are unidentifiable in terms of whole-animal taxa and in greatly varying manner some of these can be so identified reasonable. As consequence the unidentifiables generally are let alone, which probably is as it should be. If one contemplates a regulation (as objective criterion) that any unidentifiable tooth is automatically ineligible to whole-animal nomenclature but appropriate for parataxa nomenclature the situation must be faced that what is unidentifiable now may be definitely so in a few years. Considerations of this sort led us to the conclusion that objectivity in dealing with these matters could be had probably only through the Commission.

Referring to your summation in paragraph 9, I do not now see how your sub-paragraph (a) can be made effective presently by any sort of general wording. Your sub-paragraph (b) calls to mind several examples known to me that individually include questions on which the Commission should be called to rule, decision on subjective basis by an author being inappropriate. I have already discussed your sub-paragraph (c) and so pass on (d) to say that I will welcome any suggestions that meet the points you have in mind. It seems to me that present wording is susceptible of use by a zoologist " whose sole aim is to eliminate certain such names from consideration for the purposes of whole-animal nomenclature ". For example, Dr. Arkell, wishing to avoid nomenclatural bothers arising from names for aptychi could submit the same application as a paleontologist wishing to name aptychi in terms of parataxa. After studying the revised draft of our application, Dr. Arkell has written to me of his endorsement of it.

DOCUMENT 1/17

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **DON L. FRIZZELL** and **HARRIET FRIZZELL**

(*Rolla, Missouri, U.S.A.*)

(Letter dated 16th July 1957)

I find, as a micropaleontologist, that a dual classification of discrete parts and whole-animals is unavoidable. The need is obvious for numerous groups, including fish otoliths, scolecodonts, conodonts and echinoderm remains, and will be emphasized as work progresses on the taxonomy of less known microfossil forms.

In general, Moore and Sylvester-Bradley's proposals covers the situation admirably. Some points, however, may cause difficulty.

(a) It could be inhibitive to progress in micropaleontology for publication of a new parataxon type to await the Commission's ruling as to its appropriateness. Further, the need for recognition of a particular parataxon might be more apparent to active workers in the field than to members of the Commission.

To avoid delay in publication of parataxon names important to stratigraphy and paleoecology, I would urge that all parataxa be permitted, subject to protest within a limited time. The efforts of the Commission, then, would be required only in cases of actual controversy.

(b) " Discrete parts ", if possible, should be defined more adequately. For example, a bone of a fish (although obviously " discrete ") is a unit of a whole animal, whereas an otolith (or possibly a tooth) belongs unavoidably to the parataxon classification. Similarly, the gastrolith of an astacomorph decapod crustacean would be considered " discrete ", although a disjointed cheliped from the same species would be undeniably part of the whole-animal. These interpretations depend upon individual judgment, nature of available fossil material, and general usage. A *Recommendation* therefore would be preferable to stringent stipulations.

(c) It should be stressed that a parataxon classification is basically distinct in concept from a " natural " or " genetic " arrangement. It depends upon similarity of structure rather than postulated evolution. The frequent associations of biologically closely related forms is to some extent fortuitous.

For example, holothurian sieve plates occur in widely separated taxonomic groups, yet it is convenient to place them within a parataxon of family position. The otoliths of fishes show another situation. Closely related whole animals of the Recent fauna sometimes possess otoliths of types characterizing different genera and even families and in other instances a distinctive otolith type crosses generic boundaries defined from whole animals. The extra-legal nomenclatorial arrangement in use for fossil fish otoliths for nearly 75 years is similar to the proposed parataxon system, but is markedly inferior in allowing too few classificatory hierarchies.

(d) It might well be recommended that, where possible, systematists using the whole-animal nomenclature should include parataxon names in their synonymies. Parataxa, of course, should be clearly distinguished from the binomina of strictly zoological nomenclature.

(The foregoing has been discussed with Dr. Harriet Exline (Mrs. D. L. Frizzell), who concurs.

DOCUMENT 1/18

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **SAMUEL P. ELLISON, Jr.**

(*University of Texas, Austin, Texas, U.S.A.*)

(Letter dated 18th July 1957)

I have your letter and request of July 8 concerning the proposed insertion in the *Règles* of a provision defining and providing for the nomenclature of " Parataxa " (discrete parts of animals unidentifiable as belonging to whole-animal genera and species). I personally like this proposal which Dr. Moore and Mr. Sylvester-Bradley have made.

I think this would clear up a great deal of the problems of nomenclature that exist in the whole field of micropaleontology. It will also make it possible to continue the use of the Law of Priority on nomenclatorial problems that involve discrete parts of animals which have yet not been identified as to the whole.

From the view of the practical stratigrapher and micropaleontologist, I urge that your Commission adopt Dr. Moore's and Mr. Sylvester-Bradley's proposal.

PURCHASED
16 SEP 1957

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Note to Subscribers

The attention of subscribers is drawn to the fact that in order to make a start with the publication of the documents to be included in the London Agenda Paper, the present Quadruple-Part (Part 1/4) of the present volume is being published (1) before the completion of Volume 13 (the current volume containing applications for decisions from the Commission on individual names), of which the most recently published Part is Part 8 (published today), and (2) before the issue of any portion of Volume 14, the Volume earmarked for the publication of the draft English text of the " Règles " as amended by the Paris (1948) and Copenhagen (1953) Congresses. This latter volume is, however, now in the press and will be published at an early date. (intl'd) F.H., 26th August 1957.

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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AL HISTO

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CASE No. 2

ARTICLE 5 : PROPOSED CANCELLATION OF DECISION 54(1)(a) OF THE COPENHAGEN CONGRESS UNDER WHICH A FAMILY-GROUP NAME IS TO BE RETAINED WHEN BASED UPON A GENERIC NAME WHICH HAS BEEN REJECTED EITHER AS A JUNIOR OBJECTIVE, OR AS A JUNIOR SUBJECTIVE, SYNONYM OF ANOTHER GENERIC NAME

DOCUMENT 2/1

Proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **W. J. ARKELL**
(*Cambridge University, Sedgwick Museum, Cambridge*)

(Commission Reference : Z.N.(S.) 835)

(Letter prepared for the *Geological Magazine* and communicated by Dr. Arkell under cover of a letter dated 11th February 1954[1])

May I draw the attention of palaeontologists to Decision 54(1)(a) of the "Additions to, and modifications of, the *Règles Internationales de la Nomenclature Zoologique* approved and adopted by the XIVth International Congress of Zoology, Copenhagen, August 1953" ? This Decision directs that where the name of the type genus of a subfamily, family or superfamily has been changed because it is a junior synonym (whether objective or subjective), the name of the subfamily, family or superfamily based upon the name of that type genus is not to be changed.

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[1] For a fuller statement of Dr. Arkell's views see Appendix 1 to Document 2/14.

Our colleagues who sacrificed their time, energies and funds in attending the Copenhagen Congress achieved so much that is excellent and put us so greatly in their debt, that it seems ungracious to criticise their decisions. On this particular point, however, there surely must have been sharp division of opinion. The decision would have such unhappy effects in at least molluscan systematics that, if Mollusca are a fair sample, I feel palaeontologists should do all possible to prevent this clause from being incorporated in the new Rules of Nomenclature.

Some of the changes that would be required would produce completely unfamiliar monstrosities, for some family names formed on invalid objective synonyms have been jettisoned and ignored almost from the moment of proposal, along with the invalid nominal genus. The prospect of family and superfamily names henceforth having to be altered so as to revive and immortalise invalid junior synonyms is as daunting as the prospect of the discreditable hunt that will be started among the literature, to be the first to unearth these corpses for revival.

If all palaeontologists who object to this clause becoming part of the Rules will write to the Secretary of the International Commission on Zoological Nomenclature, Mr. Francis Hemming, let us hope that our protests may avail. If any systematist feels that in an exceptional case it is desirable to retain a well-known family name based on an invalid synonym it is always open to him to apply for the protection of the Commission on behalf of that name. Otherwise, the Rules surely should state the obvious : namely, that the legitimate name of a family (or subfamily or superfamily) is that which is formed on the valid name of the type genus.

DOCUMENT 2/2

Proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **SIR JOHN ELLERMAN**

(*London*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 26th February 1954 communicated by T. C. S. Morrison-Scott)

I feel very strongly against Copenhagen Decision 54(1)(a) [regarding the retention of family-group names when based on generic names which have been rejected as junior objective, or subjective, synonyms of other generic names] on account of the fact that it seems to go against established nomenclatorial practice which has been adhered to for the last century ; that it is utterly ridiculous to have a family without a genus of the same name ; superlative chaos has now been introduced [in the nomenclature of the Class Mammalia] as a result of this Ruling.

DOCUMENT 2/3

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **C. J. STUBBLEFIELD**
(*Geological Survey and Museum, London*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 6th April 1954)

I share the views printed by Dr. Arkell in *Geol. Mag.*, 1954, p. 174 : (1) that it is ungracious to criticize but (2) that it would be unfortunate to go back to the policy of allowing family names to be based on invalid junior synonyms. For instance, trilobite workers have now got used to using CYCLOPYGIDAE in place of AEGLINIDAE also OLENELLIDAE in place of MESONACIDAE and it would seem odd to recant.

DOCUMENT 2/4

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **L. R. COX**

(*British Museum* (*Natural History*), *London*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 8th April 1954)

A letter from Dr. W. J. Arkell to the Editor, just published in the *Geological Magazine*, have prompted me to write to you on the subject of Section 54 of the published Copenhagen Decisions—" the effect on the name of a taxon belonging to the Family-Group of a change in the name of its type genus ". You may remember that immediately after the Colloquium's decision, I (as one of the minority voting against it) raised the point whether its supporters intended it to be retroactive, and I pointed out that in the mollusca and other invertebrate groups with which I was best acquainted it had always been the custom to base the family name on the accepted name of the type genus and not, perhaps, on some name long previously discarded as a synonym, and that the old family names which had been discarded for reasons of synonymy or homonymy were not to be found in modern works of reference. I was astonished to learn that entomologists and ornithologists have not been in the habit of basing the family name on the accepted name of the type genus.

DOCUMENT 2/5

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **JAMES D. BUMP, MORTON GREEN, JOHN PAUL GRIES** and **J. R. MACDONALD**
(*South Dakota School of Mines & Technology, Rapid City, S. Dakota, U.S.A.*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 11th May 1954)

On page 218 of the March 1954 issue of the *Journal of Palaeontology* is a short note. " Nomenclature of Families and Superfamilies ", by W. J. Arkell. In this note Dr. Arkell comments on Decision 54(1)(a) of the "Additions to, and modifications of, the *Règles Internationales de la Nomenclature Zoologique* approved and adopted by the XIVth International Congress of Zoology, Copenhagen, August 1953 ".

It is our opinion that this suggestion should not be incorporated into the Rules as it will serve no valid purpose and add considerable confusion to many nomenclatural problems. In addition, it will invalidate the basic principle of attracting attention to the type genus of a subfamily, family, or superfamily by using the generic name as a basis for the name of the higher units.

DOCUMENT 2/6

Proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **W. H. EASTON**
(*University of California, Los Angeles, California, U.S.A.*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 12th May 1954)

Decision 54(1)(a) of "Additions to, etc.", of the Rules acted upon at Copenhagen relates to the names of families, subfamilies and superfamilies.

I am unequivocally opposed to any retroactive Ruling because it is productive of chaos. This particular Decision is doubly undesirable because it also reverses a previously understood practice which seemed to be quite reasonable.

Your co-operation is enlisted hereby to bring about such action as will remove Decision 54(1)(a) from the books.

DOCUMENT 2/7

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

(a) By **JOSEPH J. GRAHAM,** Professor **MYRA KEEN, SIEMON MULLER** and **HANS E. THALMANN**
(*Stanford University, Stanford, California, U.S.A.*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 12th May 1954)

Reference is made to the note on " Nomenclature of Families and Superfamilies " by W. J. Arkell, which was published in the *Journal of Palaeontology* **28** (no. 2) : 218, drawing attention to Decision 54(1)(a) of the "Additions to, and Modifications of, the *Règles Internationales de la Nomenclature Zoologique* approved and adopted by the XIVth International Congress of Zoology, Copenhagen, August 1953 ".

We object to this Decision which states that where the name of the type genus of a subfamily, family or superfamily has been changed because it is a junior synonym (whether objective or subjective), the name of the subfamily, family or superfamily based upon the name of that type genus is not to be changed.

We are in complete accord with W. J. Arkell that compliance with this clause will produce " completely unfamiliar monstrosities " a revival of invalid synonyms, as well as a time-wasting hunt through the literature " to unearth these corpses ".

(b) By **MYRA KEEN** and **SIEMON MULLER**

(Letter dated 29th July 1954)

As a result of correspondence with other systematists and of conversation with persons who attended the Colloquium, we wish to amend our letter of May 12th in which we supported Dr. Arkell's objections to Decision 54(1)(a). We still object in principle but are now convinced that it was the intent of the Colloquium to make this action one for the future, not the past.

If this is the case, we urge that the wording of the Decision be changed to include the date, 1953, or some later date. Only in this way can Decisions 45 and 54 be brought into harmony.

Professors Graham and Thalmann are not available for consultation at this time, but we know their stand on this issue and feel confident that they would approve this action on our part.

DOCUMENT 2/8

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **F. E. EAMES**
(*Woking, Surrey*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 13th May 1954)

I am writing to endorse and support the views expressed by Dr. Arkell in *Geol. Mag.* **91** (No. 2) : 174–175.

I greatly appreciate the good work being done by the Commission, but feel that the opinion opposed by Dr. Arkell is a retrograde step mitigating against that stability of nomenclature which is so much to be desired.

I am the Senior Palaeontologist of the Anglo-Iranian Oil Co., but the above opinions are expressed personally.

DOCUMENT 2/9

Comment on Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **CURTIS W. SABROSKY**

(United States Department of Agriculture, Agricultural Research Service, Entomology Research Branch, Washington, D.C., U.S.A.)

(Commission Reference : Z.N.(S.) 835)

(a) Statement communicated by J. Brookes Knight under cover of a letter dated 8th June 1954

W. J. Arkell (*J. Palaeont.* 28 (no. 2) : 218, March 1954), has recently attacked Decision 54(1)(a) of the " Copenhagen Decisions on Zoological Nomenclature ", which provides that a change in the name of a type genus because of synonymy, whether objective or subjective, will not necessitate a change in any family-group name founded upon it. The purpose of this decision was to provide greater stability and continuity to names of the family group, especially those of families and superfamilies that are of wide and general usefulness and importance. It is accordingly surprising to find such a worthwhile provision so vehemently opposed. If indeed the decision would produce " unhappy effects " and " completely unfamiliar monstrosities ", and if it would result in a " discreditable hunt . . . to be first to unearth these corpses [junior synonyms] for revival ", I am sure that those of us who warmly supported it at Copenhagen would join Dr. Arkell in denouncing it. But it seems clear that Dr. Arkell has misread the decision and has misjudged its effects. It is to be hoped that his forceful language has not prejudiced the case.

(1) The Copenhagen Decision actually reads " where the name of the type genus . . . has to be changed because it is found to be . . . [a junior synonym] ", and not, as Dr. Arkell gives it, where it " *has been* changed because it is . . . ." His fears of the ghosts of long-buried junior synonyms are understandable

if one begins with his verb tenses, but not with those adopted at the Congress. Decision 54(1)(a) as it stands is specifically designed to avoid changes in the future. A procedure that can be applied to the past is given in clause 45, which Dr. Arkell does not mention.

(2) Under the old Code, when the type genus was found to be a junior synonym, both generic and group names had to be changed, including those of tribe, subfamily, family and superfamily, if all were involved. Changes in at least the higher group names usually had wide repercussions in unfamiliarity and inconvenience in collateral fields and in teaching. Under the Copenhagen plan, when such synonymy occurs, the rule will not require any changes of family-group names. This will give consequent advantages in stability, continuity, and familiarity. Under Dr. Arkell's proposal, however, we would revert to the old Code, under which changes would be required in those cases. We would be forced in the future to change family-group names to those new and hence " completely unfamiliar " group names which he himself dislikes. Furthermore, in cases of subjective synonymy, we might have to change names frequently, in keeping pace with changing subjective views of authors.

(3) Even if Dr. Arkell's interpretation were correct, authors who wished to avoid exhuming old group names based on long-buried junior synonyms would have only to maintain current usage while following the procedure provided in clause 45. Surely it is a mistake to lift clause 54(1)(a) out of the plan and criticise it without relation to other provisions, especially those of clauses 45 and 54(2).

(4) It should be noted that the views of Dr. Arkell, and also those of Dr. R. C. Moore, the leader of the *Treatise of Palaeontology* currently in progress, were published in the *Bulletin of Zoological Nomenclature* prior to the Copenhagen Meeting, along with many other expressions of opinion and arguments pro and con. All of these were before the Copenhagen Colloquium, which considered the problem in detail and recommended the plan on family-group names to the Commission and thence to the Section on Nomenclature and the Congress. Palaeontologists were represented at all stages, and the views of Arkell, Moore and others were known and their viewpoints argued. There was, of course, difference of opinion on this point, as there was on many points. But the decision in clause 54(1)(a) was arrived at by the substantial majority of 20 to 8 after lengthy discussion of the advantages and disadvantages, on the first and least hurried day of the Colloquium, and under the best circumstances in which nomenclatural decisions have ever been taken.

In England, more than 25 years ago, the British National Committee on Entomological Nomenclature formally proposed that a family name was not to be changed unless it or the name of its type genus was found to be a homonym. That Committee included such well known zoologists as Karl Jordan, S. A. Neave and G. A. K. Marshall. Support for this solution of the problem has been growing in the last decade, and those who now wish to repeal it should realize the large amount of opinion on the other side. In the *Bulletin of Zoological Nomenclature* for July 1953 (**8** : Parts 6/9), in which were published the views of Arkell and Moore, there are a number of statements in favor of not changing family names in cases of generic synonymy. The proposition of not changing was supported by several groups which gave special consideration to the major problems to come before the Copenhagen Congress—namely, the Nomenclature Committee of the Society of Systematic Zoology (W. I. Follett, Chairman ; six out of eight members clearly expressing support), the American Committee on Entomological Nomenclature (C. D. Michener, Chairman), the Committee on Nomenclature of the American Museum of Natural History (John T. Zimmer, Chairman), and the Nomenclature Discussion Group of Washington, D.C. (R. E. Blackwelder, Secretary ; approved by nearly 2 to 1 majority). Palaeontologists are represented in three of those four groups. In a specialized field, the insect Order Diptera, a questionnaire sent to dipterists all over the world in 1952 showed 69 per cent. of 166 votes in favor of the solution as later adopted at Copenhagen. It thus appears that the Copenhagen vote was a fair sample of the views of zoologists.

(5) In the long view, if a great proportion of animal species remains to be discovered and named (estimates for neo-zoology alone range from 50 to 90 per cent.), the number of generic and group names yet to be proposed and shuffled about with successive classifications and reclassifications will be considerable. Any rule that will render some name changing unnecessary for the future, as does clause 54(1)(a) will be a great boon and should not be discarded.

(b) Letter dated 26th April 1956

Enclosed is a short statement on family-group names based on junior synonyms, which is a comment on Z.N.(S.) 931[1] and also on the earlier case Z.N.(S.) 811[2], as far as the general principle is concerned although not on the subject matter itself. The family-group name discussion in the *Bulletin* has been one-sided thus far, and I feel that the other side should be mentioned though detailed discussion at this time seems unnecessary.

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[1] Application Z.N.(S.) 931 here referred to was a request submitted by Dr. W. J. Arkell for the suppression under the Plenary Powers of the family-group name SEGUENZICERATIDAE Spath, 1924 (Class Cephalopoda) on the ground that that name was based upon a generic name (*Seguenziceras*) which was invalid as being a junior objective synonym of an older name (*Arieticeras* Seguenza). The application so submitted was approved by the Commission, the decision so taken being embodied in *Direction* 70 (1957, *Ops. Decls. int. Comm. zool. Nomencl.* **16** : (i)–(xii)).

[2] Application Z.N.(S.) 811 here referred to contained a request submitted by Professor Robert Mertens for the suppression under the Plenary Powers of the family-group name HATTERIIDAE Cope, 1864 (Class Reptilia), that name being based on a generic name (*Hatteria*) which was invalid as a junior synonym of an older name (*Sphenodon* Gray). This request was approved by the Commission, the decision so taken being embodied in *Opinion* 455 (1957, *loc. cit.* **15** : 379–392).

Enclosure to letter dated 26th April 1956

Recent issues of the *Bulletin of Zoological Nomenclature* have carried several references to the "unfortunate" decision by the Copenhagen Congress that a family name does not have to be changed when its type genus is found to be a synonym [Mertens, 1955, *Bull. zool. Nomencl.* **11**(5) : 139—141[3] ; Arkell, 1955, *ibid.* **11**(9) : 297—298[4] ; Jaczewski, 1955, *ibid.* **11**(9) : 304][5]. Lest the discussion appear one-sided, I am moved to point out that there is another side to the question and that there is a substantial body of opinion which believes that the decision was a wise and forward-looking contribution to stability in family-group names. To reverse that decision at the London Congress, as hoped by Mertens and Arkell, would be for many of us a retrograde step to be opposed to the utmost.

Clarification or modification of the exact application of that decision to cases in the past is, however, a different matter from reversal. I have no objection to, and will support, modifications to prevent the unfortunate and unnecessary overthrow of a long established family name in favor of one based on a junior synonym where the synonymy was established many years ago. I have not interpreted the decision as requiring such overthrow but I agree that it should be clarified wherever necessary in order to avoid such upsetting changes. But I see no reason to deprive ourselves of present and future advantages merely to conserve some past usage. There must be a better way to treat a blister than amputation of a limb.

In view of the charges that it is ridiculous and unreasonable to have family-group names based on junior synonyms, it may be interesting to point out that our sister biological field does not find it so. The latest edition of the *International Code of Botanical Nomenclature* (as adopted at the 7th International Botanical Congress, Stockholm, July 1950 ; published 1952, Utrecht) states in Article 28 that " The name of a family is a plural adjective used as a substantive taken from the name of its type genus or from a synonym, and ending in -aceae ". Article 29 makes the same provision for subfamily names. The previous edition (1935) read as follows : " Article 23. Names of families are taken from the name of one of their present or former genera and end in -aceae ". The wording of the 1935 edition is not as appropriate as in the 1952 revision, but the same meaning was intended, as is clear from the example cited in both editions : " Caryophyllaceae (from Caryophyllus, a pre-Linnean genus) " [*Caryophyllus* of post-Linnean authors is cited in synonymy under *Dianthus* Linnaeus]. As in certain other matters, notably the status of names cited in synonymy, zoologists could well profit by adopting the wise provisions of the Botanical Code.

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[3] The application here referred to is that cited in footnote 2 above.

[4] The application here referred to is that cited in footnote 1 above.

[5] The communication here referred was a note of support by Professor Tadeusz Jaczewski for the application by Professor Mertens cited in footnote 2 above.

(c) Letter dated 7th February 1957

I herewith submit comments on the proposed use of the Plenary Powers for GAVIIDAE.[6] The comments are on the general principles concerned with family-group names, and not on the bird names *per se*, the latter being out of my field.

In paragraph 7, it is good to note the comment that Copenhagen Decision 54(1) (a) had a praiseworthy object and could serve that purpose in the future. I do not believe, however, that the deplored re-emergence from synonymy of long-buried family names is a necessary result of that Decision, nor do I believe that it is necessary to invoke the Plenary Powers. Authors who dig up such names in challenge to long-established usage are borrowing trouble unnecessarily and, one might almost suspect with malice aforethought to put the Decision in the worst possible light. I suggest two alternatives :—

(a) That authors exercise the option provided in Copenhagen Decision 45, which was written for the precise purpose of avoiding resort to the Plenary Powers as much as possible ;

(b) That authors defer usage-upsetting changes or applications regarding them until the London Congress has had an opportunity to clarify the Decision, and in the meantime follow the long honored principle of maintaining the *status quo* until such review has been carried out. I confidently believe that many applications are and will be found to be unnecessary, and can and should be avoided at this time in the interests of economy of effort and publication space.

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[6] For a fuller reference to the proposal here mentioned by Dr. Sabrosky see paragraphs 4 and 5 of Document 2/14.

DOCUMENT 2/10

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **R. M. STAINFORTH**

(*International Petroleum Company, Talara, Peru*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 15th June 1954)

I have just noted Dr. Arkell's note in the March *Journal of Paleontology* relative to the status of family names. He protests at a decision of the XIVth International Congress of Zoology at Copenhagen (Decision 54(1)(a)) that family names should not be changed when the name of the type-genera are changed by synonymy.

I agree heartily with Dr. Arkell. An instance arose very recently in connection with a foraminiferal paper which I have been editing for Dr. J. Hofker. He formerly used the family name PARRELLIDAE, based on the foraminifer *Parrella*. However, *Parrella* has since been shown to be preoccupied for a fish. It seems to me that the word PARRELLIDAE must automatically name a family of fishes, if it is to have any valid meaning at all, and the foraminiferal family must be re-named according to the name which replaces the invalid one—in this case OSANGULARIIDAE from *Osangularia*. That is rather a case of homonymy and perhaps has no bearing on the clause at issue, but my protest remains. Picking up one of the older foraminiferal works at random, I see the subfamily POLYSTOMELLINAE (in Flint, 1899). By the new ruling this (unless preceded by a still older name) would be valid for the NONIONIDAE or ELPHIDIINAE of modern authors, even though the synonymy of *Polystomella* (1822) and *Elphidium* (1808) has been so long established that the former is no more than a faint ghost from the past. That example was on the first page I looked at, and I shudder to think of the hundreds of similar cases which must exist, and at the chaos which would result from attempting to apply rules of priority to the family names.

DOCUMENT 2/11

Support for Dr. W. J. Arkell's proposal for the withdrawal of Copenhagen Decision 54(1)(a)

By **C. A. FLEMING** and **N. DE B. HORNIBROOK**
(*Geological Survey, Wellington, New Zealand*)

(Commission Reference : Z.N.(S.) 835)

(Letter dated 2nd August 1954)

We wish to record our sympathy with the opinions expressed by Dr. W. J. Arkell in his letter to the *Geological Magazine* (**91**(2) : 174—175) and his note in *J. Pal.* **28**(2) : 218 concerning the provisions of Decision 54(1)(a) of the " Additions to, and Modifications of, the *Règles Internationales de la Nomenclature Zoologique* approved and adopted by the XIVth International Congress of Zoology, Copenhagen, August, 1953 ". We believe that the name of a family, subfamily and superfamily should be based on the valid name of the type genus.

DOCUMENT 2/12

Proposed maintenance of Copenhagen Decision 54(1)(a)

By Members of the Committee on Entomological Nomenclature,
Entomological Society of America

(Commission Reference : Z.N.(S.) 835)

(Communicated by Robert L. Usinger, under cover of a letter
dated 4th November 1954)

During the past year two questions were referred to the Committee. These concerned actions taken by the International Commission at Copenhagen and were raised by palaeontologists. The questions are stated below :—

1. Strong objection has been taken in the palaeontological field to the decision that " where the name of the type genus of a taxon belonging to the Family-Group has to be changed because it is found to be either (i) a junior objective synonym or (ii) a junior subjective synonym, the name of the Family-Group taxon based upon the name of that type genus is not to be changed " (*Cop. Dec. Zool. Nomencl.* : 36, paragraph 54(1)(a)).

. . . .[1]

The E.S.A. Committee voted on upholding the Copenhagen Decisions as follows :—

Question 1 : 6 for, 1 against.

. . . .

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[1] The second portion of this Resolution deals with a different subject (the practicability of applying the priority principle to family-groups) and will be published later as the present volume as part of the documentation relating to Case No. 5 on the London Agenda Paper.

DOCUMENT 2/13

Views of the Nomenclature Committee of the Society of Systematic Zoology

(Commission Reference : Z.N.(S.) 835)

(Letter, with enclosures, from W. I. Follett to the Secretary to the Commission, dated 19th September 1956)

A majority of the Nomenclature Committee of the Society of Systematic Zoology favors retention, subject to a provision excluding retroactivity, of the decision reached at Copenhagen on the subject of type genera in synonymy. The following comprises the Committee's correspondence to date on this subject :—

ANNEXE 1 TO DOCUMENT 2/13

Circular letter dated 7th August 1954 by W. I. Follett, Chairman, to the Members of the Nomenclature Committee of the Society of Systematic Zoology

Mr. Sabrosky has suggested that we consider the objection that has recently been expressed to the following decision rendered at Copenhagen :

> Where the name of the type genus of a taxon belonging to the Family-Group has to be changed because it is found to be either (i) a junior objective synonym or (ii) a junior subjective synonym, the name of the Family-Group taxon based upon the name of that type genus is not to be changed. *Copenhagen Decisions on Zoological Nomenclature*, 1953 : 36 Sec. 54(1)(a).

Objection to this provision has been published by Dr. W. J. Arkell (*Journal of Paleontology* 28(2), 1954 : 218 ; *Geological Magazine* 91(2), 1954 : 174).

To this objection, Mr. Sabrosky has replied by a two-page mimeographed statement. Mr. Sabrosky has since received a letter from Dr. L. R. Cox, of the British Museum, who believes that the provision in question must be retroactive to 1758, and who states, " The International Rules as a whole were made retroactive to 1758, although not codified and legalized until very long afterwards, and any additions to them must be considered to be similarly retroactive unless a specific statement is made to the contrary and a definite date fixed from which they are to apply (e.g. the date 1930, after which a diagnosis is required for a new genus to be valid)."

The problem necessarily involves the question of reversal of a decision rendered at an International Congress of Zoology, and whether such a reversal should be confined to a demonstrable error of fact or whether it may properly include a decision rendered after publication of the opposing views and after discussion and vote at the Congress.

Will you please tell me whether you favor retention or reversal of the Copenhagen Decision quoted above. An expression of your views on this problem will also be useful.

ANNEXE 2 TO DOCUMENT 2/13

Comments by Curtis W. Sabrosky

(a) Letter dated 3rd September 1954

I believe that there is another alternative which should be considered, namely :—

(1) To retain the Copenhagen provision as it stands, retroactive to 1758 (assuming Hemming and Cox to be technically correct, regardless of what we may have thought we were doing).

(2) To reject that decision, and thus to return to the provisions of the old Code. (Arkell's position.)

(3) To amend the decision so as to preserve its action for the future, but to provide for the past in some other way to avoid confusion and upsetting changes (e.g., my proposal for committee of specialists, cf., *Bull. zool. Nomen.* **8** (Parts 6/9) : 176).

My personal view is, and has always been, that the third choice is the best. Accordingly, I should like to propose that as an alternative rather than voting for either retention or reversal. I understand that because of various difficulties now apparent, such as the accurate determination of the prior name for each group (c.f. Usinger's second question), Hemming has also come to the view that the approach by specialists' committees and the preparation of *Official Lists* would be the best way to proceed.

(**b**) Correspondence between W. I. Follett and Curtis W. Sabrosky

(i) Letter from W. I. Follett to Curtis W. Sabrosky
dated 28th October 1955

In attempting to surmount a mass of unfinished business, I have come to your problem of type genera in synonymy. You will recall that under date of August 7, 1954 I wrote the members of the S.S.Z. Nomenclature Committee on this subject, enclosing a copy of your two-page mimeographed reply to Arkell's objection.

Few members of the committee have replied to my letter and I propose to call the matter to the attention of those who have not done so, and to request that they express their view within a specified period. Before writing such a follow-up letter, I should like to have any suggestions that may occur to you. Particularly, do you wish to comment on (1) Article 5, Section, of Bradley's draft (" No new rule shall retroact in such manner as to overturn the well-established usage of any name ") and (2) footnote 7 of my " Unofficial Interpretation . . . ", suggesting that the protest procedure be applied to this situation ?

(ii) Letter from Curtis W. Sabrosky to W. I. Follett
dated 8th November 1955

Re yours of 28th October ; I have little to add beyond my letter of 3rd September 1954. I had a nice exchange of letters with Arkell, and we agree that this problem will undoubtedly have to be settled at the London Congress. But it would be worth while, I think, to give consideration to it and to place some reaction on record with Hemming. My position in the controversy with Arkell is certainly in keeping with the provision in Bradley's draft. I favor the third choice in my letter of 3rd September rather than the procedure suggested in your footnote 7, chiefly because the latter would drag the business on and on, involve the Commission in endless details, and, unless the *Bulletin of Zoological Nomenclature* can be much more widely disseminated than it is now, zoologists are not going to pay too much attention anyway. I favour a positive approach, to get the job done.

ANNEXE 3 TO DOCUMENT 2/13

Comment by Cyril F. dos Passos

Letter dated 10th August 1954

In answer to your air mail letter of 7th August, you are advised hereby that I favor the retention of the *Copenhagen Decisions on Zoological Nomenclature*, 1953 : Decision 54(1)(a). I concur fully with the views expressed by Mr. Sabrosky's enclosed memorandum on this subject.

ANNEXE 4 TO DOCUMENT 2/13

(a) Comments by Professor Myra Keen

(a) Letter dated 9th August 1954

Before we had received the printed copy of the Copenhagen Decisions we responded to the plea of Dr. W. J. Arkell—taking it at face value—and wrote a letter, signed jointly by Dr. Graham, Dr. Müller, and Dr. Thalmann and myself, to Mr. Hemming protesting the supposed provisions of Sec. 54(1)(a). Later, Dr. J. Brookes Knight saw a copy of this letter, which apparently was circulated by Mr. Hemming, and Dr. Knight wrote us an urgent request that we withdraw our letter. By this time we had the *Copenhagen Decisions* in hand and after studying them and the arguments of Dr. Knight and Dr. Sabrosky, Dr. Müller and I, in the absence of the other two signers, wrote Mr. Hemming as follows : " . . . We still object in principle, but are now convinced that it was the intent of the Colloquium to make this action one for the future, not the past. If this is the case, we urge that the wording of the clause be changed to include the date 1953 or some later date. Only in this way can Decisions 45 and 54 be brought into harmony." We asked Mr. Hemming, therefore, to amend our previous letter in the event he planned to publish it. For my own part, to make Sec. 54 retroactive to 1758 would seem to me to be in direct contradiction to Sec. 45, and I should object most strongly to any such provision. As I told Dr. Knight, it would be carrying priority to the point of absurdity.

(b) Letter dated 10th December 1955

Concerning your letter of 7th December to the members of the Nomenclature Committee of S.S.Z. : I have nothing to add to my previous letter of August 1954, except the observation that I remain opposed to the general attempt to bring family-group names under the Rules. The problems of ranking, content, and spelling are so numerous that it seems to me the work involved in searching the literature is all out of proportion to the precision gained. Moreover, the Commission is already burdened with enough real problems without having to make decisions upon these more or less artificial ones.

ANNEXE 5 TO DOCUMENT 2/13

Comments by ERNST MAYR

(a) Letter dated 11th August 1954

I strongly endorse Curtis Sabrosky's stand. I disagree entirely with Cox since it was stated again and again at the Colloquium that provisions to be adopted at the meetings were to be valid from the day of publication of the

Copenhagen Decisions and could obviously not be retroactive. The Decision in question has incorporated the future tense " has to be changed " which as Curtis shows clearly proves that this is not a retroactive provision. However, I would assume that if a well-known family name has been changed in recent years on the basis of Article 5 that authors will be free to apply to the International Commission for an exercise of its Plenary Powers. I have no objection if Messrs. Arkell and Cox ask for a statement confirming the non-retroactive powers of the new Article, but I would vigorously fight any attempt to repeal one of the most stabilizing decisions adopted by the Copenhagen Congress.

I do not quite feel ready to make a formal proposal concerning Paragraph 46 of the *Copenhagen Decisions* until I have had some reaction. Up to now I have had very numerous requests for reprints but no reaction except from Hemming who states that the Article, incorporating priority for family names, has raised such formidable technical difficulties that it may be best to drop it. Having tried myself to determine the first date of proposal of several classical family names, I can only sympathize with Hemming. Perhaps it would be better if you as Chairman of our Committee would request an expression of opinion either from the members of the Committee or if you should prefer, from the Society as a whole as to their reaction.

(b) Letter dated 12th December 1955

Thank you for your communication on Copenhagen Decision 54. Nothing has happened that would induce me to change my endorsement of Sabrosky's interpretation of the *Copenhagen Decisions* and his proposal. I believe American courts pay a good deal of attention to the intention of a given law or constitutional provision. In the present case there is no question whatsoever that the intention of the Copenhagen law makers was to stabilize nomenclature with the new provision. This automatically rules out the proposal to revoke the *Copenhagen Decisions* and to go back to the old Article 5 with its continuous changing of family names for purely nomenclatural reasons on the generic level. This, likewise, automatically rules out the Arkell interpretation which, I believe Mr. Hemming half-heartedly adopts that this is a retroactive provision. As Dr. Arkell says quite correctly a retroactive proposal would play havoc with stability. Consequently this leaves only one interpretation of Copenhagen Decision 54 namely that it should become valid and binding as of the date of publication. Any other interpretation would seem legalistic, ritualistic and unquestionably contrary to the intent of the Copenhagen Congress.

ANNEXE 6 TO DOCUMENT 2/13

Comment by Carl L. Hubbs

Letter dated 13th December 1955

It seems to me rather absurd that we should give serious consideration to the argument of Cox, that the new Rule should be retroactive to 1758. That certainly wasn't intended and doesn't make sense. It seems to me to be entirely in keeping with the spirit of our actions and the spirit of the times to make the small addition of fixing a definite date when this new rule will take effect. We decided to have a date for several actions, and to leave the designation of the date up for future determination. This would be in line with Sabrosky's third alternative proposed on September 3, 1954, though I do not think that we need to go with him and make everything dependent on what a committee of specialists will do with the fixing of family names. This is going to be a difficult job, and in some groups it will be hard to get a sound set of specialists together. In some groups the job may be done so poorly as to be obviously bad. I don't believe that we should object to having specialists go into the matter, in the hopes that for a good many groups the job will be done and done well. However, we will need to face the fact that such action will often not be taken. So I think that in recommending the appointment of such committees, there should be some provision that the matter can be settled in some other way ; I would say preferably in line with the proposal in your Footnote 7 of the ' unofficial interpretation '.

DOCUMENT 2/14

Report on the action taken by the International Commission under its Plenary Powers to prevent the application of Copenhagen Decision 54(1)(a) (retention in certain cases of family-group names based upon invalid generic names) in certain cases where it had been represented by specialists in the groups concerned that otherwise confusion and name-changing would result

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Commission Reference : Z.N.(S.) 835)

The purpose of the present Report is to give particulars of the action taken by the International Commission under its Plenary Powers to prevent the application of Copenhagen Decision 54(1)(a) (retention of family-group names based upon generic names rejected as junior objective, or as junior subjective, synonyms of other generic names) in certain cases where it has been represented that otherwise objectionable disturbance of current nomenclatorial practice would be involved. These cases are instructive as showing how in actual cases Copenhagen Decision 54(1)(a) would not only fail to secure the object for which it was devised—the stabilisation of family-group-name nomenclature—but would in fact defeat that object by promoting undesirable name-changing.

(a) The family-group names " Arieticeratinae " Howarth, 1955, and " Seguenziceratidae " Spath, 1924

(Class Cephalopoda, Order Ammonoidea)

" Opinion " 337 and " Direction " 70

2. In October 1950 Dr. W. J. Arkell (*Sedgwick Museum, Cambridge University, Cambridge*) submitted to the International Commission an application in which he asked that a Ruling be given rejecting the reputed generic name *Arieticeras* Quenstedt, 1883 (Class Cephalopoda, Order Ammonoidea) and in consequence (i) accepting as an available name the name *Arieticeras* Seguenza, 1885, and (ii) rejecting as a junior objective synonym of that name the generic name *Seguenziceras* Levi, 1896 (which had been published as a replacement name for *Arieticeras* Seguenza in the belief that that name was invalid under the Law of Homonymy). This application was approved by the Commission, whose decision in this matter was later embodied in *Opinion* 337 (1955, *Ops. Decls. int. Comm. zool. Nomencl.* **10** : 109—124).

3. At the time when Dr. Arkell submitted the foregoing application to the Commission the family-group-name implications involved were not dealt with, the *Official List of Family-Group Names in Zoology* not having at that time been brought into existence. When, subsequent to the Copenhagen Congress, this case was reviewed from the foregoing point of view, it was found that under Decision 54(1)(a) of that Congress the family-group name SEGUENZICERATIDAE Spath, 1924, would remain the valid family-group name for *Arieticeras* Seguenza, 1885, notwithstanding the fact that under the Ruling given in *Opinion* 337 the generic name *Seguenziceras* Levi, 1896, had been rejected as a junior objective synonym of *Arieticeras* Seguenza and had been placed on the *Official Index of Rejected and Invalid Generic Names in Zoology*. Dr. Arkell who had from the first expressed the strongest opposition to Copenhagen Decision 54(1)(a)[1] at once submitted to the Commission a supplementary application asking that the Plenary Powers should be used to restore the position by suppressing the family-group name SEGUENZICERATIDAE Spath, thus leaving the way clear for the acceptance of the family-group name ARIETICERATINEA Howarth, 1955, based upon the valid name for the type genus of this family-group taxon. An extract from Dr. Arkell's supplementary application is annexed to the present paper as Appendix 1. The application submitted by Dr. Arkell was approved by the Commission, the decision so taken being later embodied in *Direction* 70 (1957, *Ops. Decls. int. Comm. zool. Nomencl.* **16** : (i)—(xii)).

(b) The family-group names "Gaviidae" Coues, 1903, and "Urinatoridae" (correction of "Urinatores)" Vieillot, 1818 (Class Aves)

" Opinion " 401 and " Direction " 75

4. In 1950 the Standing Committee on Ornithological Nomenclature which had then recently been established by the International Ornithological Congress, submitted to the International Commission an application designed to bring to a close a controversy centred around the generic name *Colymbus* Linnaeus, 1758 (Class Aves) which for seventy years had divided the ornithologists of the Old World and the New, the former using this name for the Divers (Loons), the latter for the Grebes. The proposal so submitted was that the Commission should use its Plenary Powers to cut the Gordian Knot by suppressing the name *Colymbus* Linnaeus as a name hopelessly compromised and deprived of utility by long-standing discordant usage, thus clearing the ground for the use of the foregoing genera of names which were both well-known and not subject to any doubt as to their interpretation. Under this proposal the name *Gavia* Forster, 1788, would become the valid generic name for the Divera (Loons) and *Podiceps* Latham, 1787, the valid generic name for the Grebes. This application was published in 1952 (*Bull. zool. Nomencl.* **9** : 6—7) and received a wide measure of support from ornithologists. At the close of the Prescribed Waiting Period the foregoing proposals were unanimously approved by the Commission. The decision so taken was later embodied in *Opinion* 401 (1956, *Ops. Decls. int. Comm. zool. Nomencl.* **13** : 1—64).

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[1] For a paper published on this subject by Dr. Arkell, see Document 2/1.

5. At the time of the submission of the application upon which the foregoing *Opinion* was based the *Official List of Family-Group Names in Zoology* had not as yet been established and in consequence the family-group name aspects of the *Colymbus* case were not then considered. When, however, this matter came to be examined, it was found that at the family-group name level the settlement reached in *Opinion* 401 would in part be upset if the provisions of Copenhagen Decision 54(1)(a) were to be applied, for, if that Decision were to have been so applied, it would have been impossssible to employ the family-group name GAVIIDAE Coues, 1903, for the Divers (Loons), it being necessary under that Decision to disinter the long-buried name URINATORIDAE (correction of URINATORIDES) Vieillot, 1818, a name of older date based upon the name *Urinator* Lacépède, 1799, a name long rejected as a junior subjective synonym of *Gavia* Forster, 1788. This unfortunate development was brought to notice in a note by myself as Secretary (1956, *Bull. zool. Nomencl.* **12** : 240—247), in which I suggested that, in order to harmonise the position at the family-group-name level with that established at the generic-name level by the action taken by the Commission under the Plenary Powers in *Opinion* 401, the family-group name URINATORIDAE Vieillot, 1818, should be suppressed under the Plenary Powers, thus restoring to the Divers (Loons) the family-group name GAVIIDAE Coues, based upon the generic name which in the foregoing *Opinion* the Commission had validated for that group. No objection to this course was received by the Commission from any source,[2] and the foregoing recommendation was unanimously approved by the Commission. The decision so taken has since been embodied in *Direction* 75 (1957, *Ops. Decls. int. Comm. zool. Nomencl.* **13** : 291—308).

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[2] The application here referred to did, however, evoke from Dr. Curtis W. Sabrosky a further note on Copenhagen Decision 54(1)(a), the text of which has been reproduced as Document 2/9(c).

(c) The family-group names "Upogebiinae" Borradaile, 1903, and "Gebiidae" (correction of "Gebidae") Dana, 1852, and "Processidae" Ortmann, 1896, and the names "Nikidae" Bate, 1888, and "Hectarthropidae" Bate, 1888 (Class Crustacea, Order Decapoda)

"Opinion" 434

6. In May 1954, Dr. L. B. Holthuis (*Rijksmuseum van Natuurlijke Historie, Leiden, The Netherlands*) placed before the International Commission a request for the validation under the Plenary Powers of the well-known generic names *Upogebia* [Leach], [1814], and *Processa* Leach, [1815] (Class Crustacea, Order Decapoda). In each of these cases there was a family-group name based on the generic name in question that was in common use but which would under Copenhagen Decision 54(1)(a) disappear in synonymy, for in each case there was an older family-group name based upon a generic name which was a junior synonym—in the first of these cases, a junior subjective synonym, in the second, a junior objective synonym, of the name of the type genus in question. In the first case the name UPOGEBIINAE Borradaile, 1903, was a junior objective synonym of GEBIIDAE (correction of GEBIDAE) Dana, 1852, a nominal taxon having as its type genus *Gebia* Leach, 1815, the name of which was a junior objective synonym of *Upogebia* [Leach], [1814]. In the second of the cases referred to above there were two family-group names involved. These were NIKIDAE Bate, 1888, and HECTARTHROPIDAE Bate, 1888. In each of these cases the name of the type genus was a junior subjective synonym of *Processa* Leach, [1815]. Thus, under Copenhagen Decision 54(1)(a) the name PROCESSIDAE Ortmann, 1896, would need to be sunk as a junior subjective synonym of NIKIDAE Bate, 1888, or, if that name were to be suppressed under the Plenary Powers, of HECTARTHROPIDAE Bate, 1888, unless that name were also to be so suppressed. In these cases the Commission was asked to validate the names in common use (UPOGEBIINAE and PROCESSIDAE) by suppressing under its Plenary Powers the older names based on discarded generic names (in the first case, the senior objective synonym GEBIIDAE ; in the second case, the senior subjective synonyms NIKIDAE and HECTARTHROPIDAE), thus validating the established practice of carcinologists in regard to these names. This proposal was approved by the Commission, the decision so taken being embodied later in *Opinion* 434 (*Ops. Decls. int. Comm. zool. Nomencl.* **14** : 403–424).

(d) The family-group names "Sphenodontidae" Cope, 1870, and "Hatteriidae" Cope, 1864 (Class Reptilia)

"Opinion" 455

7. In November 1954, Professor Dr. Robert Mertens (*Senckenbergische Naturforschende Gesellschaft, Frankfurt a.M., Germany*) submitted to the International Commission an application having as its principal object the

validation of the currently accepted emendation to *Sphenodon* of the generic name originally published by Gray (J.E.) in 1831 with the defective spelling *Sphaenodon* (Class Reptilia). The problem raised by Copenhagen Decision 54(1)(a) was involved in this case also, for the universally-used family-group name SPHENODONTIDAE Cope, 1870, would under that Decision fall as a junior objective synonym of HATTERIIDAE Cope, 1864, a nominal taxon having as its type genus *Hatteria* Gray (J.E.), 1842, which has the same type species as, and is therefore objectively identical with, *Sphenodon* (correction of *Sphaenodon*) Gray (J.E.), 1831. In this application Professor Mertens asked that the confusing and objectionable change which would result from the application of Copenhagen Decision 54(1)(a) in this case should be prevented from taking place by the suppression by the Commission under its Plenary Powers of the long-rejected name HATTERIIDAE Cope, 1864, thus validating the name SPHENODONTIDAE Cope, 1874, in general use for the family concerned. An extract from Professor Mertens' application setting out his views on this matter is given in Appendix 2 to the present note, while in Appendix 3 is given a note on the same subject which was furnished by Professor Dr. Tadeusz Jaczewski (*Zoological Institute, Polish Academy of Sciences, Warsaw, Poland*) after the publication of Professor Mertens' application in the *Bulletin of Zoological Nomenclature*. The proposal submitted in this matter was approved by the Commission which under its Plenary Powers validated the name SPHENODONTIDAE Cope, 1870, by suppressing the name HATTERIIDAE Cope, 1864. The decision so taken has since been embodied in *Opinion* 455 (1957, *Ops. Decls. int. Comm. zool. Nomencl.* **15** : 379–392).

(e) No proposals received for the application of Copenhagen Decision 54(1)(a) in particular cases

8. In conclusion, it should be added that in no case submitted to the Commission has the applicant asked that, in accordance with the provisions of Copenhagen Decision 54(1)(a), a currently-accepted family-group name should be rejected in favour of some family-group name of older date that had been discarded as being based upon a generic name which was a junior synonym (either objective or subjective) of the name of the type genus of the currently accepted family-group taxon.

APPENDIX 1 TO DOCUMENT 2/14

Objection to Copenhagen Decision 54(1)(a)

By **W. J. ARKELL**
(*Cambridge University, Sedgwick Museum, Cambridge*)

Extract from application in regard to the family-group names
SEGUENZICERATIDAE Spath, 1924, and ARIETICERATINAE
Howarth, 1955

(Arkell, 1955, *Bull. zool. Nomencl*, **11** : 297–298)

. . . .

4. In the present case the difficulty arises from the unfortunate decision by the Copenhagen Congress that a family-group name is not to be rejected when the name of its type genus is rejected as being (as in the present case) a junior objective synonym of some other generic name (1953, *Copenhagen Decisions zool. Nomencl.* : 36, Decision 54(1)(a)). I have already protested strongly against this decision (1954, *Geol. Mag.* **91** : 174, 410 ; 1955, *J. Paleont.* **29** : 188)[3] which in my group and, as I now learn, in many other groups must, if applied, lead to the overturning of many well-known family-group names in favour of other names which have passed out of use completely. It is my strong hope that the next (Fifteenth) International Congress of Zoology, when it meets in London in 1958, will reverse the foregoing decision in favour of the long-established practice under which a change is made in a family-group name when it is found necessary to change the name of its type genus because that name is either a junior objective synonym or a junior subjective synonym of some earlier name. In the present case, I consider that it would be ridiculous if there were to be an available name SEGUENZICERATIDAE in view of the fact that by the Ruling given by the Commission *Seguenziceras* Levi, 1896, is a junior objective synonym of *Arieticeras* Seguenza, 1885.

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[3] For the paper here referred to see Document 2/1.

APPENDIX 2 TO DOCUMENT 2/14

Objection to Copenhagen Decision 54(1)(a)

By **ROBERT MERTENS**
(*Natur-Museum u. Forschungs-Institut Senckenberg, Frankfurt a.M., Germany*)

Extract from application in regard to the generic name
Sphenodon Gray (J.E.), 1831

(Mertens, 1955, *Bull. zool. Nomencl.* **11** : 140–141)

. . . .

6. We have now to consider the third of the family-group names concerned, namely HATTERIIDAE Cope, 1864 (*Proc. Acad. nat. Sci. Philad.* 1864 : 227) the type genus of which is *Hatteria* Gray, 1842, which, as shown above, is a junior objective synonym of *Sphenodon* Gray, 1831. The name SPHENODONTIDAE Cope, 1870, which is in current use for this family, is junior by six years to the name HATTERIIDAE Cope, 1864. Up to 1953, this would not have prevented the family in question from being known by the family name (SPHENODONTIDAE) based upon the oldest valid name for its type genus. Unfortunately, however, the Fourteenth International Congress of Zoology, Copenhagen, 1953, when revising the rules relating to family-group names, inserted a provision that " Where the name of the type genus of a taxon belonging to the Family-Group has to be changed because it is found to be either (i) a junior objective synonym or (ii) a junior subjective synonym, the name of the Family-Group taxon based upon the name of that type genus is not to be changed " (1953, *Copenhagen Decisions on Zoological Nomenclature* : 36, Decision 54(1)(a)). This decision represents a most unfortunate innovation and one calculated to cause much name-changing and confusion at the family-name level, and it is much to be hoped that it will be reversed by the next (London, 1958) International Congress of Zoology. In the present case this decision if applied, would lead to the rejection of the well-known family name SPHENODONTIDAE and its replacement by the long-rejected and inappropriate name HATTERIIDAE. In present circumstances the only way by which this result can be avoided is for the Commission

to use its Plenary Powers to suppress the family-group name HATTERIIDAE Cope, 1864, thereby validating the accepted name SPHENODONTIDAE Cope, 1870. This course the Commission is therefore now asked to take.

. . . .

APPENDIX 3 TO DOCUMENT 2/14

Objection to Copenhagen Decision 54(1)(a)

By **TADEUSZ JACZEWSKI**

(Institute of Zoology, Polish Academy of Sciences, Warsaw, Poland)

Letter dated 12th June 1955 furnished in support of Robert Mertens' application regarding the generic name *Sphenodon* Gray, 1831

(Jaczewski, 1955, *Bull. zool. Nomencl.* **11** : 304)

I wish to support in full extent the proposition by Professor Dr. Robert Mertens, concerning the validation of the emended generic name *Sphenodon* Gray, 1831, and of the family-group name SPHENODONIDAE Cope, 1870.

In particular I wish to support most strongly the opinion expressed by Professor Dr. Mertens in paragraph 6 (: 140—141) of the above proposition concerning the use of family-group names derived from generic names which proved to be synonyms. I quite agree with Professor Dr. R. Mertens that it should be hoped that the unfortunate Copenhagen Decision 54(1)(a), will be finally reversed by the next International Congress.

In my opinion the only way leading to a reasonable stability of family-group names is through the previous stabilisation, i.e., placing on the *Official List*, of the names of the corresponding type genera of the families in question. I think that the mentioned Copenhagen Decision is manifestly contrary to the very principle of the type method which is taken as the basis for the formation of family-group names. I am unable to see any reasonable idea in the maintenance of family-group names derived from synonymous generic names which may be placed any time on the *Official Index of Rejected and Invalid Generic Names in Zoology*, and become thus doomed to oblivion. I think valid family names based on invalid generic names are a very anomalous combination.

DOCUMENT 2/15

Proposed withdrawal of Copenhagen Decision 54(1)(a)

(Commission Reference : Z.N.(S.) 835)

By **W. F. WHITTARD**

(*University of Bristol, England*)

(Letter dated 26th September 1957)

I have recently had occasion to consult Decision 54(1)(*a*), reached by the International Congress of Zoology at Copenhagen, with regard to the trilobite family group-name ALASTASPIDAE Turner, 1940. Whittington (1952) correctly claimed that *Alsataspis* Turner, 1940, as a subjective junior synonym of *Seleneceme* Clark, 1924 ; accordingly he replaced the family group-name ALSATASIDAE by SELENECEMIDAE. The Decision quoted above rules the name SELENECEMIDAE to be invalid. This Decision appears to me to be at fault when, as a result, it perpetuates in the family group-name an incorrect and false generic name founded on an invalid synonym. I agree with Arkell[1] that the name of a family is only legitimate when it is founded on a valid generic name.

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[1] See Document 2/1.

CASE No. 3

ARTICLE 5 : PROPOSED AMENDMENT AND/OR CLARIFICATION OF DECISION 54(1)(b) OF THE COPENHAGEN CONGRESS REGARDING THE DATE AND AUTHORSHIP TO BE ATTRIBUTED TO THE NAME OF A FAMILY-GROUP TAXON PUBLISHED AS A SUBSTITUTE FOR A FAMILY-GROUP NAME REJECTED BY REASON OF THE NAME OF ITS TYPE GENUS BEING A JUNIOR HOMONYM OF AN OLDER GENERIC NAME

[**Editorial Note :** Mr. G. H. E. Hopkins (*British Museum (Natural History), The Zoological Museum, Tring, Herts*) has intimated his intention of submitting a proposal for the amendment of Copenhagen Decision 54 (1)(b) so as to provide that a substitute family-group name shall be treated as possessing the same priority and authorship as the name which it replaces. This paper, which is not yet available, will be published in the present series as soon as it is received.]

DOCUMENT 3/1

Request for a clarification of Copenhagen Decision 54(1)(b)

By **A. K. MILLER** and **W. M. FURNISH**

(*State University of Iowa, Iowa City, U.S.A.*)

(Commission Reference : Z.N.(S.) 1252)

(Letter dated 15th April 1954)

In a recent circular, Professor R. C. Moore asked that you be sent as soon as possible an expression of views from individual *Treatise* authors concerning matters of family-group nomenclature to be formulated in the new *Règles*.

It seems to us that Decision 54(b) of the Copenhagen Decisions should be clarified because it does not specify (1) the authorship or (2) the date of establishment of a Family-Group taxon the name of which is " replaced by a name based upon the changed name of the type genus ". This becomes important in case a subjective synonym for the Family-Group was proposed during the interim between the original proposal and the change.

DOCUMENT 1/19

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **H. SCHMIDT**

(Geologisch-Palaeontologisches Institut der Georg August-Universität, Gottingen, Germany)

(Letter dated 17th July 1957)

Zu Ihrer Anfrage (Z.N.(S.) 1056) darf ich zunächst auf meinen Brief vom 3.7.56 verweisen. Zeile 5–8 desselben ist überholt.

Ich meine, dass Parataxa in der Paläontologie in grosser und rasch anwachsender Zahl vorhanden sind. Sie verursachen Störungen in der Nomenklatur, und ein Versuch zu Anpassung der I.R.Z.N. an die Lage erscheint angebracht. Deshalb stimme ich dem Grundgedanken des Vorschlages Moore/Sylvester-Bradley bei. Für die Fassung des vorgeschlagenen Artikels halte ich jedoch gewisse Änderungen für richtig :

Im Absatz (a) empfehle ich zu streichen " by decision of the Commission ". Die Verantwortung dafür, ob die Beziehung auf ein ganzes Tier möglich ist oder nicht, kann und soll, wie ich meine, den Autoren nicht abgenommen werden. Es ist schon jetzt gelegentlich besonders bei den Conodonten so, dass Fragmente bearbeitet und benannt werden, ohne dass der jeweilige Autor die Beziehung auf ein ganzes Tier sucht, obwohl es Möglichkeiten dazu gibt. Wird den Autoren die Verantwortung in dieser Richtung abgenommen, so könnte die heute noch verbreitete Zurückhaltung durch eine Inflation von Namen abelöst werden. Das Bewusstsein, dass jedes Parataxon ein unerwünschter Notbehelf ist, sollte erhalten bleiben.

Zum Absatz (b) meine ich auch, dass die Freiheit, neue Gruppen von Paratax-Namen zu schaffen, beschränkt werden sollte. Wenn aber die

Kommission nach dem Vorschlag Moore/Sylvester-Bradley 1956 Ziffer 3 (pag. 2/3) verfährt, wird das nicht genügen, weil zahlreiche Parataxa für Fährten und Spuren berücksichtigt werden müssen, darunter auch solche incertae classis. Also würde erst eine Zeitspanne benötigt werden, um die Zulassung der Parataxa aus den vorgesehenen Tierklassen durchzuführen. Danach müssten für weitere Tierklassen Anträge gestellt und bearbeitet werden. In der Zwischenzeit könnte es Verwirrung und Unzufriedenheit geben. Ich habe 1956 permische Reptilfährten bearbeitet und fand dabei, dass ohne einige neue Namen eine ausrichende Diskussion der Befunde nicht möglich war. So ergibt sich wiederum der Rat, man möge die Aufstellung neuer Parataxa nicht ermuntern, aber auch nicht hindern.

An Stelle des Abstazes (b) würde ich demgemäss lediglich eine Ermahnung an die Autoren vorschlagen, bei der Schaffung neuer Parataxa soweit möglich Zurückhaltung zu üben.

Den Absatz (c) empfehle ich zu unveränderter Annahme.

Unter " Ratschläge " könnte beigefügt werden : Parataxa-Gattungsnamen sollten zusammengesetzte Wörter sein, bei denen ein Bestandteil den Begriff als Parataxon kennzeichnet. Dazu sind Affixe verwendbar, wie- ichnus, -aptychus etc. Auch die Verwendung eines kurzen Präfixes ist zulässig, diese erscheint für Verwendung in alphabetischen Listen vorteilhaft.

Mit dem letzten Ratschlag beziehe ich mich auf Erfahrungen der Palynologen : Die Verabredung von 1951 (Heerlen), die Namen der Sporae dispersae stets mit -sporites (-pollenites) zu verbinden, hat sich bewährt, wenn auch einige Wörter von monströser Länge die Folge waren. Bei meinen Fahrten hätte ich gern statt Harpagichnus etwa Pe-Harpargis und statt Palmichnus Pe-Palmae, geschreiben aber mein Vorschlag in *Paläont. Z.* **28** (1954, pag. 3) ist wohl noch zu isoliert, als dass schon jetzt das nötige Verständnis hätte erwartet werden können.

DOCUMENT 1/20

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **JOHN W. KOENIG**

(*Missouri Geological Survey & Water Resources, Rolla, Missouri, U.S.A.*)

(Letter dated 22nd July 1957)

Dr. Don L. Frizzel of the Missouri School of Mines and Metallurgy has been most kind in bringing to my attention the contents of your letter pertaining to the proposed insertion in the *Règles* of a provision for " Parataxa ". Although my work with microcrinoids and bryozoans does not ordinarily call for the use of a parataxonomic system, I am often aware of the fact that the wealth of currently useless disconnected parts of whole animals which I encounter in my samples could be effectively used in the solving of many stratigraphic problems if such a system as proposed by Moore and Sylvester-Bradley were to be erected for their reception.

If the system is properly initiated, it should prove to be an excellent tool for all paleontologists and stratigraphers. To this end, I would like to emphasize the following points :

(1) The definitions of the several types of discrete parts and life stages to be considered by the commission as parataxonomic in nature should be set forth as explicitly as possible so that there can be no confusion as to what is to be included. Special care should be taken in the consideration of life stages of whole (or more specifically) mature animals. For example, the very immature forms in the growth series of at least two general of Paleozoic microcrinoids, *Allagecrinus* and *Kallimorphocrinus* could conceivably be considered as parataxa because at this stage in their development it is exceedingly difficult and often impossible to determine to which adult genus they belong. However, it would be unnatural and cumbersome to place these forms in a taxa different from that of the adults.

(2) Unavoidable delays in Commission Rulings on newly proposed parataxa may prove to be more of a hindrance than an aid to active workers primarily concerned with the use of such units. It would be more practical to allow the publication of such names subject to protest within a reasonable period of time.

(3) The need for absolute distinction between parataxonomic and taxonomic nomenclature cannot be stressed strongly enough. There should be no question as to the immediate recognition of parataxonomic names when used with descriptions, discusssions, and faunal lists of whole animals. It also would be very useful if all paleontologists were urged to include parataxa in their synonymies of whole-animals when and if the relationships had been established.

DOCUMENT 1/21

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

Views of the Scientific Staff of the Australian Museum, Sydney, Australia

(Letter dated 26th July 1957 communicated by the Director
of the Museum, **J. W. EVANS**)

I circulated your letter of July 8th, to the scientific staff of this Museum. Most of them have no particular comments to make, but I give below those of Mr. Whitely, Curator of Fishes.

The proposed provision of Parataxa is largely a matter for palaeontologists.

Sometimes fish otoliths of uncertain allocation are given scientific names such as " *Gadidarum laevigatum*, sp. nov." in which *Gadidarum* is not a proper generic name but indicates that the new species of otolith can only be assigned at present to some genus or other of the family GADIDAE.

When dealing with Recent fishes, ichthyologists usually express unidentifiable novelties as " Gen. indet." or by some similar term. There are not very many *genera incertae sedis* amongst Recent fishes ; these are mostly loose scales, otoliths, sharks' teeth, etc.

I cannot recall any eggs of Recent fishes having been named as genera or species. Larvae such as Leptocephalus have been named and their names accepted or rejected according to the *Règles* as their adults have been associated with them.

Leigh-Sharpe (*J. Morph.* **39** : 567–573) studying claspers of skates (Raia) named a number of new " pseudogenera ", as he called them, framing for each a new name by prefixing a Greek letter to the Latin word *Raia* : thus *Alpharaia*, *Betaraia*, etc. These " pseudogenera " were provided with type-species by Leigh-Sharpe, and were acknowledged as subgenera by Jordan (1925, *Copeia* **142** : 37) who resolved them into subgenera when valid and synonyms of subgenera which had been named earlier—a satisfactory arrangement.

It might be better for parataxa to be clearly named as such in their place of original publication, and for their names to be independent of the Law of Homonymy, in the manner of a name published for some purpose other than for use in zoological nomenclature (*Copenhagen Decisions*, 1953, p. 63, para. 114(1)).

In general, I see no objection to the proposed scheme as outlined in this circular Z.N.(S.) 1056, except that I feel that the decision to classify an object as a taxon or parataxon should be the responsibility of its describer, as a specialist in his field, rather than of the Commission.

DOCUMENT 1/22

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **R. S. BASSLER**

(*Washington, D.C., U.S.A.*)

(Letter dated 29th July 1957)

In reply to your letter and enclosed Moore/Sylvester-Bradley proposals of 5th July 1957 (Z.N.(S.) 1056) I have conferred with our local students deeply concerned in the fundamental laws of zoological nomenclature. All agree that further complicating the Rules would be inexcusable and might lead to disrespect for decisions of the International Commission. The basic Rules have been reprinted so often by scientific institutions and private individuals and the methods for correcting mistakes are so ample that there is no lack of information about them.

It would have been wise had Moore presented his subject for at least preliminary discussion in say the *Journal of Paleontology* or other American publications before his proposals were sent to the Commission. However, as has been well said in American politics " Let us look into the record ".

F. A. Bather, 1900 (Echinoderms in Ray Lankester's *Treatise on Zoology*) changes the name of over 75 genera of crinoidea and cystoidea without any explanation or previous references. American authors rebelled so in 1924 a conference held at Washington with Bather, Ullrich, Foerste and Bassler present led to the agreement that one of us should change the names back to the original valid ones. Naturally it fell to the youngest member and Bassler did so in 1938 and 1943. Moore and Laudon in their large crinoid work following Bassler adopted all these valid names, a procedure followed by succeeding authors and text-book writers. Moore, however, as early as 1939 (*Denison Univ. Bull. J. Sci. Lab.*) proposed an elaborate classification of fragmentary crinoidal remains which some of us strongly disagreed with. Then as noted by Bassler in 1943 (*Bibliographic and Faunal Index*, Pal. Echin. : 2) Moore acknowledged that the fragments had no value as genera and species in classification. These fragments probably gave rise to the later term PARATAXA.

In 1957 Moore, with a European sponsor (Mr. P. C. Sylvester-Bradley), proposes the insertion in the *Règles* of the new term " parataxa ", evidently to replace his fragmentary fossil remains of 1939 and 1943. Considering the trouble that parataxa will cause in our legitimate classification and the approval-responsibilities placed upon the Commission, this is certainly the time for all good naturalists to come to the aid of our taxonomy.

I contend that the use of common good sense can relieve the difficulties of classification whether it be cephalopod or conodont. Any name based upon an aptychus can remain until the whole shell is known, whereupon the aptychus-name can go in parenthesis labelled as the operculum. Conodonts can be treated likewise until the entire animal is found, maybe centuries later, but the old unlocated names must be held for stratigraphic reasons. In such cases the Commission at no time should make final decision except in species with completed case-history.

Summing up, I feel that the proposals cited are necessary so seldom that there is no need of expanding their discussion.

DOCUMENT 1/23

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **R. V. MELVILLE**

(*Geological Survey and Museum, London*)

(Enclosure to letter dated 9th August 1957)

Palaeontologists have long felt that some of the difficulties inherent in their material should be recognised by the incorporation of additional provisions in the *Règles.* The Moore/Sylvester-Bradley proposals are to be welcomed as the first successful attempt to direct the Commission's attention to these difficulties.

2. I feel, however, that stricter drafting would better fit the proposals to the palaeontological purposes for which they are designed. Consideration must first be given to the level at which the criterion of " unrecognisability " is to be applied. For example, in the Class Echinoidea, Order Cidaroida, scores of species and a number of genera have been based on detached radioles ; in many of these cases the corona (which is generally held to represent a whole-animal species in fossil echinoids) is not known ; in many other cases, however, the whole-animal species is known and can be identified from detached radioles. I have not investigated how conflicts between synonymous names in this category have been resolved in arriving at the currently stable usage, but clearly there is no call for exceptional legislation. Some whole-animal species of Diadematacean and Echinacean echinoids can also be determined from detached radioles, but in most cases such fragments could only be recognised at family or order level, while radioles of Gnathostomatan and Atelostomatan echinoids could in general only be recognised as such.

3. Some whole-animal species of fishes can be identified from detached teeth, otoliths, scales or fin-spines, although in other cases these discrete parts

can only be recognised at some higher taxonomic level. Gislén (1924 ; *Zool. Bidr. Uppsala* 9) has proposed names for certain post-Palaeozoic articulate crinoids that are known only from discrete parts ; here, although the whole-animal species is not known, at least the ordinal placing is certain. Some whole-animal species in this group can be determined from discrete parts.

4. In cases of the kind mentioned in the two preceding paragraphs, it does not seem to me that any special rules are called for, since even if the whole-animal species is not known, authors have clearly considered that the remains under consideration have represented some whole-animal species for which it was necessary to have a name. Either it is possible to identify the discrete-part species with some existing nominal species, or it can be given a new specific name in an existing nominal genus, or it can be given a new binomen in an existing family-group taxon (or placed *incertae sedis* in some existing order/class-group unit). The course adopted would depend on the level at which the discrete-part taxon could be recognised in terms of whole-animal taxa. I am convinced that all such names should be treated as coordinate in all respects with the names of true taxa, and I should certainly oppose any attempt to treat such nominal species of echinoids as those mentioned in paragraph 2 above in any other way.

5. The special difficulty with the conodonts is that even the conodont-assemblages cannot be placed in an existing phylum or class. For the sake of argument, however, it can be assumed that the units distinguished among these assemblages are whole-animal species, whose nomenclature should be governed by the *Règles* in force at any given time.

6. The greatest difficulty arises with discrete-part fossils that are known not to represent whole-animal taxa, but for which it is necessary to have a system of nomenclature of one sort or another. Dissociated conodonts and the aptychi of ammonites are fossils of this kind. Remains of this sort may be of the greatest scientific importance to palaeontologists, since they may be the most useful fossils in an assemblage from the point of view of stratigraphical dating of the strata and of making correlations from one place to another ; they may even provide the sole evidence on which the marine or non-marine nature of the sediments is to be judged. I consider that the total suppression of all names based on ammonite aptychi, as recommended by Dr. Arkell (*Bull. zool. Nomencl.* **9** : 266–268), would be a disservice to those palaeontologists who need names by which to record the occurrence of these fossils and to make deductions based on such evidence.

7. It is clearly necessary in the interests of palaeontologists that the nomenclature of discrete-part fossils that are known not to represent true taxa should be uniform and stable. It seems preferable to me for this to be

achieved through the International Commission rather than otherwise. Any independent authority that was set up to deal with this difficulty would be under heavy pressure to deal also with other problems that afflict palaeontologists which the International Commission has hitherto refused to treat in the manner requested. This could only lead to the establishment of a palaeontological commission in competition with the existing authority—a proceeding already canvassed among certain palaeontologists. This would be a disastrous result.

8. What in effect is desired is that the International Commission should extend its functions to cover the regulation of the nomenclature of certain entities known to be outside the scope of zoological nomenclature as that term is currently defined. The fossils mentioned in paragraphs 6 and 7 above—and I would confine the term " parataxa " to these categories alone—bear names that would be thought of as " technical terms " in current definitions. These names form part of the international language of zoology and they are used to communicate concepts just as the names of normal taxa are used. Nevertheless, since the boundary between the nominal concepts represented by these names and those represented by names presently governed by the *Règles* is subjective and liable to fluctuation, it seems to me essential that the two categories of names should be subject to the rule of the same authority.

9. The Moore/Sylvester-Bradley proposals for the government of the nomenclature of parataxa seem to me adequate and effective. It is obviously desirable to eliminate homonymy between the names of parataxa and the names of normal taxa, so that both categories of names should be subject to the same Law of Homonymy. It is also necessary for the two categories to be independent from the point of view of the Law of Priority, since in the nature of parataxa it will always be unlikely that any one of them will be found to be synonymous with any one normal taxon. In cases where specialists are agreed that a condition of synonymy exists between one nominal parataxon and one taxon, it should be ruled that the name of the taxon should always prevail.

DOCUMENT 1/24

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Letter dated 12th August 1957)

Professor Moore and Mr. Sylvester-Bradley kept me informed as to their proposals regarding parataxa while they were working on them. I was very favourably impressed. I at once saw that any such proposal, if adopted, would also have to apply to the non-taxonomic groups that parasitologists are permitted to use, by exception. I urged Mr. Sylvester-Bradley to communicate his plan to Dr. Stoll.

Since I am neither a paleontologist nor a parasitologist, I do not feel competent to commit myself as to the value and practicability of the plan. I do not feel that the Commission should, or could, drive paleontologists and parasitologists into adopting such a plan, and I think that it would be premature to adopt it as part of the *Règles* in 1958. It might, however, be wise to announce that if any considerable number of specialists adopt it in their own work within the next five years, that consideration would be given at the next succeeding Congress to incorporating appropriate provisions in the *Règles*.

Since the problem facing parasitologists and that facing paleontologists seems fundamentally the same, I should be opposed to making in the Code separate exceptional solution for each discipline.

I would suggest that a footnote be added to the first paragraph of Professor Moore's Digest, hanging on the words " Whole Animals ", to the following effect.

" Whole Animals " is to be interpreted broadly, to mean not only all parts of an individual, but also all life-stages, sexes, and forms of a species.

The last sentence of paragraph 6 of Professor Moore's Digest frightens me. The Commission lacks the necessary specialised knowledge.

I believe that (perhaps permanent) committees of paleontologists and parasitologists would have to be set up to advise the Commission.

The Recommendations made in paragraph 7 of Professor Moore's Digest appear to me adequate and acceptable.

DOCUMENT 1/25

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **CARL C. BRANSON**

(*Oklahoma Geological Survey, Norman, Oklahoma, U.S.A.*)

(Letter dated 12th August 1957)

In regard to the proposal of Moore and Sylvester-Bradley to recognise " parataxa " :

I am opposed to any departure from rigid adherence to the *Règles* except in a relatively few calamitous instances. The case of *Trigonellites* versus *Oppelia* I regard as one. Nomenclature is to be thought of as to be used for all time and not as a matter for the convenience of the present generation.

Any application of the proposal to conodont taxonomy is decidedly premature. No difficulties arise from the present system excepting in the attempts of Rhodes and of Scott to except " true genera ". Scott's assemblages are caprolite associations. The validity of other assemblages is not demonstrated.

It is my opinion that the *Règles* should not be changed in this respect.

DOCUMENT 1/26

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **ROBERT O. FAY**

(*Oklahoma Geological Survey, Norman, Oklahoma, U.S.A.*)

(Letter dated 12th August 1957)

In regard to your letter of July 8 in which you express a desire for comments on the proposed insertion in the *Règles* of a provision for the nomenclature of parataxa, I have these questions.

(1) On page 3, last paragraph[1], how does one know that *Scottognathus* is a whole-animal species ? If *Scottognathus* is a whole-animal species, how do we know that all conodonts are whole-animal species ? As all conodont specialists well know, the fibrous conodonts (which would be treated as parataxa) may be different from other (lamellar) conodonts, and may have to be treated as whole-animal taxa. This renders the *Règles* useless in this case because there is room for doubt, even within a group of specialists. This means that we will treat lamellar conodonts as whole-animal taxa when found in assemblages and as parataxa when found as discrete components but we must admit complete ignorance with the fibrous conodonts so in our ignorance we must blindly make a decision. I vote to treat the fibrous conodonts as whole-animal taxa, but now we cannot call them conodonts, because lamellar conodonts are parataxa. However, others may not share my viewpoint and may wish to treat fibrous conodonts as parataxa, thus placing them with the lamellar conodonts. I have no serious objections to this latter plan except that it is not objective or based on fact. *Archeognathus* Cullison (1938) may be a fibrous conodont, and if so there is good reason to classify this group as whole-animal taxa. Many people may not wish to accept *Archeognathus* as a fibrous conodont

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[1] The paragragh here referred to will be found on pages 104-105 of Quadruple—Part 1/4 of the present volume.

and thus ruin my basis for classification, and thus use the *Règles* to fit the person. At present there is no competent person to make a decision on the classification of conodonts (either taxa or parataxa) due to the fact that we lack fundamental facts concerning the anatomical position, systematic relations, and physiological functions of the conodonts. We are proceeding along an odd path, but all specialists know that *Scottognathus* is a name for an assemblage and *Hindeodella* is a name for a discrete unit. Therefore, I vote that we dismiss this proposed insertion of parataxa into the *Règles*, because it is not sound, premature, and unnecessary, and highly subjective.

(2) Who will make decisions on trilobites, etc. ?

DOCUMENT 1/27

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **J. ROGER**

(*Muséum National d'Histoire Naturelle, Paris*)

(Enclose to letter dated 14th August 1957)

Le problème est d'une très grande importance générale et pour les spécialistes des divers groupes se présente, dans ses aspects pratiques sous, des aspects différents. Parmi les paléontologistes qui travaillent dans mon service, sur les Ammonites, les Bryozoaires, les Lamellibranches, les Gastropodes etc. . . . nous avons discuté cette proposition. C'est le résultat de ces échanges de vues que, très sommairement, j'indique ci-dessous :

Le problème de la nomenclature, ainsi qu'il a été posé dans la proposition de R. C. Moore et de P. C. Sylvester-Bradley, est absolument fondamental, et cela non seulement pour la paléontologie mais aussi pour la taxonomie des êtres vivants et actuels. If faudrait reprendre l'historique du développement des sciences biologiques, spécialement systématiques, depuis une centaine d'années pour retirer l'impression d'une sorte de ralentissement des progrès et peut-être même d'un enlisement depuis une vingtaine d'années. Les causes en sont multiples. Parmi celles-ci, il semble que la manque d'adaptation de la taxonomie au besoins nouveaux de la recherche biologique systématique soit d'une grande importance. N'y-a-t-il pas d'ailleurs un désaccord fondamental qu'il convient de souligner : les règles de base de la taxonomie ont été fondées par Linné, qui n'était pas évolutionniste et depuis près de cent ans, de façon de plus en plus unanime, la biologie se développe sur la base philosophique évolutionniste. Le problème posé déborde donc largement le cadre de simple loi formelle de nomenclature, mais passe aussi dans le domaine de la philosophie scientifique, c'est-à-dire qu'il intéresse non seulement la situation présente, mais sa solution peut jouer un rôle déterminant dans l'orientation future des recherches de Biologie systématique.

Il semble aussi que le problème de la nomenclature des pièces ou éléments séparés d'un organisme doive être discuté en commun entre paléontologistes, zoologistes de toutes les spécialités et botanistes. Autrement dit il faut une solution globale. En effet, nécessairement nous sommes amenés à effectuer des comparaisons et à établir des identités ou des différences entre les groupes actuels et les groupes fossiles, entre la systématique d'une grande catégorie donnée avec celle d'une autre. La validité de telles confrontations dépendra donc essentiellement de l'unicité de la conception systématique. Cela d'ailleurs est d'autant plus valable que même pour les groupes d'êtres vivants actuels où en principe on dispose, pour définir les catégories systématiques, de l'ensemble de l'organisme on n'utilise le plus souvent que les caractères se rapportant à certaines parties du corps qui, généralement, ne sont pas celles que la fossilisation conserve.

Ces quelques commentaires, qui demanderaient de beaucoup plus longs développements, tendent seulement à prouver l'importance de la question posée.

Pratiquement l'échange de vues que nous avons eu dans mon laboratoire est tout d'abord favorable à la distinction proposée d'une taxonomie et d'une parataxonomie. La difficulté majeure sera évidemment de savoir ce qu'il convient de classer dans la catégorie " taxon " et dans la catégorie " parataxon ". Il semble que pour chaque groupe du règne vivant les spécialistes devront se mettre d'accord.

On pourrait envisager deux cas : (1) Les pièces séparées décrites peuvent être identifiées quant à leur signification dans l'organisme globale. (2) Ces pièces sont énigmatiques ou de signification douteuse.

Dans le premier cas on peut utiliser le binome linnéen habituel en le faisant suivre de la désignation de l'appareil ou de l'organe auquel appartient la pièce en question. Les objets de cette nature seraient donc soumis à la taxonomie normale. D'ailleurs cela mériterait encore une large discussion et pour les restes par trop fragmentaires il serait probablement nécessaire de ne pas adopter la loi de priorité.

Pour la seconde catégorie il y aurait lieu d'appliquer les recommandations données dans la note de R. C. Moore et P. C. Sylvester-Bradley.

En tout état de cause l'adoption de ces propositions suppose la mise sur pied de toute une organisation technique. En effet :

(a) Il y aura lieu de marquer la délimitation entre " taxon " et " parataxon " pour chaque catégorie d'animaux ;

(b) De nombreux cas particuliers dont la solution urgente sera une des conditions essentielles de succès de la réforme, se trouveront posés. L'application d'une parataxonomie suppose un fonctionnement accéléré des commissions de nomenclature ;

(c) Une très large documentation systématique sans cesse tenue à jour par un dépouillement très complet de la littérature zoologique, paléontologique et aussi géologique sera nécessaire.

DOCUMENT 1/28

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **M. F. GLAESSNER**

(*University of Adelaide, Adelaide, S. Australia*)

(Letter dated 16th August 1957)

The proposals contained in the document concerning Parataxa have been considered by the Palaeontological Seminar of this University [Adelaide University] under my direction and are found appropriate and acceptable. We have decided to support them. I shall pass on the document to other interested Australian palaeontologists who may not have received it, and you will hear from them directly.

DOCUMENT 1/29

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **O. M. B. BULMAN**

(*Department of Geology, University of Cambridge, Sedgwick Museum, Cambridge*)

(Letter dated 19th August 1957)

I am afraid I have no experience of Parataxa and no comments to offer on the plan submitted by Moore and Sylvester-Bradley, Z.N.(S.) 1056. I have read it through and the proposals seem to me eminently sane.

Anything that will tend to nomenclatorial stability has my blessing.

DOCUMENT 1/30

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **FREDERICK M. SWAIN**

(*University of Minnesota, Minneapolis, U.S.A.*)

(Letter dated 4th September 1957)

I acquiesce to the proposals for nomenclature of " Parataxa " introduced by Professor R. C. Moore and Mr. P. C. Sylvester-Bradley.

In my collections are some *Miocene cladoceran ephippia* that cannot at present be placed in an existing genus and I shall gladly follow recommendations of Moore and Sylvester-Bradley in regard to naming these parts.

DOCUMENT 1/31

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **O. H. SCHINDEWOLF**

(*Institut und Museum für Geologie und Paläontologie der Universität Tübingen, Germany*)

(Enclosure to letter dated 9th September 1957)

Den von R. C. Moore & P. C. Sylvester-Bradley gestellten Antrag, den Begriff der " associate taxa " oder " parataxa " in die *Règles* einzuführen und deren Nomenklatur verbindlich zu regeln, halte ich für sehr gefährlich. Ich lehne ihn aus folgenden Gründen ab :

1. Isolierte Einzelteile (" discrete parts ") und Gesamttiere (" whole animals ") sind im Bereiche der Paläontologie relative Begriffe und nicht scharf gegeneinander abgrenzbar. " Whole animals " gibt es unter den Fossilien überhaupt nicht. Das Ammoniten-Gehäuse kann dem Ammoniten-Aptychus nicht als " whole animal " gegenübergestellt werden. Es fehlen die Weichteile, und wenn wir den Bau des Ammoniten-Tieres nach Analogie des rezenten *Nautilus* auch einigermassen rekonstruieren zu können glauben, so wissen wir doch nichts Sicheres selbst über die für die Stellung im System so grundlegende Frage, ob es vier Kiemen wie beim *Nautilus* oder nur zwei wei bei den Dibranchiaten besessen hat. Noch weniger können die Conodonten-Vergesellschaftungen im Hinblick auf die einzelnen Conodonten-Formen als " whole-animals " gelten ; denn hier ist über die zugehörigen Tiere und über die anatomische Stellung der Conodonten im Tierkörper rein gar nichts bekannt.

In einigen seltenen Ausnahmefällen verfügt der Paläontologe über Fossilien mit mehr oder weniger vollständig erhaltenen Weichteilen, aber dann fehlen immer noch die ursprüngliche Farbe, die Ontogenie und, da es sich um tote Organismen handelt, Anhalte für das ethologische und ökologische Verhalten und manches andere mehr, das streng genommen zur exakten taxonomischen Festlegüng gehört. Bei den (bisher stets ohne Weichteile überlieferten) Ammonoideen kann es auf Grund zahlreicher neuer Beobachtungen keinem Zweifel unterliegen, dass ihre Gehäuse zum mindesten in vielen Fällen gefärbt waren. Die Arten aber sind durchweg auf nachtraglich entfärbte Gehäuse begründet. Es kann da der Fall eintreten, dass neue Stücke gefunden werden, die einer beschriebenen Art durchaus entsprechen, sich jedoch durch zwei verschiedene Farbmuster unterscheiden. In einem solchen Falle ist es völlig unmöglich anzugeben, für welche Form der auf gebleichte Ammoniten-Gehäuse basierte Artname zu übertragen ist, d.h. es kann hier grundsätzlich der gleiche Sachverhalt einer Nicht-Identifierbarkeit bestehen wie bei den " Parataxa " mit Bezug auf die Taxa der " whole animals ".

Alle unsere Fossilien sind grundsätzlich unvollständig ; für die Taxonomie bedeutet es keinen prinzipiellen Unterschied, ob nun $\frac{3}{4}$, $\frac{1}{2}$, $\frac{1}{4}$ oder nur 1/10 des ursprünglichen Gesamttieres vorliegt, vorausgesetzt, dass diese Teile als charakteristsch und identifizierbar gelten dürfen. Belanglose Einzelteile, die aller Wahrscheinlichkeit nach keinerlei diagnostische Merkmale besitzen, d.h. artlich und gattungsmässig nicht wiedererkannt werden können, wird ein verantwortungsbewusster Systematiker überhaupt nicht benennen.

2. Bei der gegenwärtig geltenden Rechtslage stehen für die Benennung unvollständiger Fossilien bzw. isolierter Einzelteile zwei Wege offen : (a) Sie werden mit einer echten Nomenklatur belegt, wobei Art. 27a der *Règles* Anwendung findet. (b) Es wird fur sie sie eine morphologische bzw. anatomische Terminologie angewendet, die den *Règles* nicht unterliegt. Diese beiden Möglichkeiten dürften für alle Fälle völlig ausreichend sein.

Die Zulassung von " Parataxa " wird ein ungeheures Anschwellen unserer Nomenklatur zur Folge haben, ohne dass irgendein nennenswerter Vorteil dadurch erzielt würde. Vor allem besteht keiner hinreichende Veranlassung für eine Sonderbennenung der Einzelteile, sobald deren Zugehörigkeit bekannt ist. Der Antrag von Moore & Sylvester-Bradley sieht vor, dass nur für eine begrenzte Anzahl von Tiergruppen die Einführung von " Parataxa " durch die Nomenklatur-Kommission genehmigt werden soll. Es ist indessen mit Sicherheit vorauszusehen, dass zahlreiche weitere Anträge für andere Tiergruppen folgen werden. Der eine Autor wird " Parataxa " für die Pygidien und Hypostome der Trilobiten fordern ; andere werden sie wünschen für die Stiele der Pelmatozoa, für die häufig isoliert gefundenen Ambulacralia, Adambulacralia, Circumoralia, Terminalia usw. der Asteroidea, für die Femora, Humeri, Wirbel und Zähne (möglichst gesondert für alle Einzelzähne !) der Säugetiere und für zahlreiche weitere Skelettelemente der Tetrapoden. Die Folgen für die Ausweiteung der Nomenklatur und für die Beanspruchung der Nomenklatur-Kommission wären nicht abzusehen, wenn man hier überall " Parataxa " zulassen würde, was für die einzelnen Skelettelemente der Vertebraten mit demselben Recht gefordert werden könnte wie für die Aptychen der Ammoniten und für die Conodonten.

3. Hinsichtlich der Aptychen der Ammonoidea, mit denen ich mich etwas näher beschäftigt habe, möchte ich die folgendenBemerkungen machen :

(a) Die Aptychen bzw. Anaptychen sind, was allerdings in diesem Zusammenhang weniger wichtig ist, Opercula der Gehäuse von Ammonoidea und ganz auf diese Ordnung beschränkt. Sie können nicht, wie Moore & Sylvester-Bradley für möglich halten, auch zu " belemnoids and soft-bodied dibranchiates " gehören, da diese kein Aussengehäuse besitzen. Auch als etwaige ererbte Rudimente sind sie bei den Dibranchiaten nicht zu erwarten, da sich diese nicht von den Ammonoideen ableiten.

(b) Nicht zutreffend ist der Satz von Moore & Sylvester-Bradley : " No one has been willing to accept the senior name of an aptychus as substitute for the name of a conch, despite stipulation of the *Règles* (Art. 27) that the first published name for a part of an animal shall be recognised for designation of the whole animal ". H. Matern (*Senckenbergiana*, **13**, 1931, S. 163) beispielsweise hat auf Grund des älteren Aptychen-Namens *Spathiocaris koeneni* Clarke, 1884 den Gehäuse-Namen *Crickites holzapfeli* Wdkd., 1913 in *Crickites koeneni* (Clarke) umbenannt.

(c) Moore & Sylvester-Bradley empfehlen in ihrem Antrage, eine grosse Anzahl der von F. Trauth für die Aptychen vorgeschlagenen " Gattungs "-bzw. " Typus "- und " Art "-bzw. " Form "-Namen zu übernehmen und als " Parataxa " zuzulassen. Es kann jedoch keinem Zweifel unterliegen, dass die Benennungsweise der Aptychen durch Trauth völlig ausserhalb der gültigen Nomenklaturregeln steht. Er hat die alten bestehenden Namen *Tellinites*, *Trigonellites*, *Solenites*, *Ichthyosiagones* verworfen und dafür im Interesse einer einheitliche Bezeichnungsweise neue, mit -aptychus zusammengesetzte Namen eingeführt. *Synaptychus* Fischer wird als angeblich wenig bezeichnender Name unter Missachtung der Priorität durch *Striaptychus* Trauth ersetzt, *Sidetes* Giebel, 1847 als " Subgenus " bzw. " Subtypus " von *Anaptychus* Oppel, 1856 behandelt. Trauth hat ferner nicht verfügbare Homonyme verwendet, willkürlich die Typen Aptychen- " Gattungen " verschoben, Namen für noch nicht vorhandene, später vielleicht aufzufindende Formen vorgeschlagen. In zahlreichen Fällen wurden von ihm neue " Gattungs "- und " Artnamen " für die Aptychen altebenannter Ammonitenformen aufgestellt, was bis heute glücklicherweise unzulässig ist. *Cornaptychus* beispielsweise ist nach der späteren Typenfestsetzung durch Moore & Sylvester-Bradley begründet auf den Aptychus von *Hecticoceras hecticum* (Rein.), *Granulaptychus* auf den von *Garantiana* aff. *suevica* Wetz., *Pseudostriaptychus* auf den von *Parapachydiscus pseudostobaei* (Mob.), *Rugaptychus* auf den von *Baculites knorrianus* (Desm.) usw. Neue, von Trauth vorgeschlagene " Artnamen " sind unter vielen anderen : *Anaptychus carapax angustus* für den Aptychus von *Psiloceras planorbis* (Sow.), *A. mitraeformis* für den von *Pleuroceras spinatum* (Brug.), *A. pala* für den von *Amaltheus margaritatus* (Montf.), *A. sellaeformis bicarinatus* für den von *Asteroceras stellare* (Sow.), *A. latexcisus* für den von *Lytoceras cornucopiae* (Y. & B.). In einigen Fällen tragen bei Trauth die " Artnamen " der Aptychen die Genitivform der Artnamen des zugehörigen Gehäuses: *Cornaptychus hectici, Striaptychus spinigeri*. Das kann doch nur soviel bedeuten, dass es sich hier um die besondere Aptychus-form des *Hecticoceras hecticum* (Rein.), bzw. *Acanthoscaphites spiniger* (Schlüter) handelt, mit denen man sie im Zusammenhang gefunden hat.

Da Trauth nach diesen wenigen Proben die derzeit gültigen Nomenklaturregeln und das Prioritätsprinzip völlig ausser acht gelassen hat, kann seine Benennungsweise, ganz gleichgültig was seine eigene Intention gewesen sein mag, nicht als gültige Nomenklatur, sondern lediglich als eine unverbindliche Terminologie bewertet werden. Das gleiche gilt für die Bezeichnungen anderer Autoren, beispielsweise für die " Gattung " *Neoanaptychus*, die T. Nagao 1931 für den Aptychus von *Gaudryceras tenuiliratum* Yabe, 1903 aufstellte.

(d) Die Termini *Cornaptychus, Granulaptychus, Laevaptychus, Striaptychus* usw. sind zweifellos nützlich, um diese besonders gestalteten Aptychen-Typen zu kennzeichnen. Sie sollten jedoch als rein morphologische bzw. strukturelle Begriffe (" technical terms "), die ausserhalb der Nomenklatur stehen, behandelt und daher auch als " Parataxa " nicht zugelassen werden. Als reine Termini berühren sie ohnehin die Nomenklatur der Ammoniten-Gehäuse und ihre Priorität nicht, so dass der gefährliche Weg einer Einführung von " Parataxa " nicht beschritten zu werden braucht. Für eine besondere " artliche " Benennung der Aptychen bekannter, bereits benannter Ammoniten-Arten besteht keinerlei Bedürfnis. Aptychen unbekannter Zugehörigkeit mögen vorläufig binär bezeichnet werden, wie es ja auch bei anatomischen Benennungen vielfach üblich ist. Der Name entfällt, sobald die Zugehörigkeit erkannt worden ist.

(e) Das einzige, was zu geschehen hat, um die Gehäuse-Namen der Ammoniten zu schützen und die Nomenklatur vor unnötigen Verwirrungen zu bewahren, ist die Unterdrückung einiger älterer, nomenklatorisch gültig aufgestellter Aptychen-Namen wie *Trigonellites, Ichthyosiagones* und *Sidetes*, die Priorität gegenüber später aufgestellten Gattungsnamen für Ammoniten-Gehäuse besitzen. Die neuren Aptychen-Namen, die auf bereits benannte Ammoniten begründet sind, stehen ohnehin ausserhalb der Linnéischen Nomenklatur und können daher nicht in Konflikte mit ihr geraten. Soweit gelegentlich nomenklatorische Schwierigkeiten auftauchen sollten, können sie jedenfalls durch Suspension der " Regeln " weit einfacher und weniger folgenschwer behoben werden als durch Einführung einer " Parataxonomie ".

DOCUMENT 1/32

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **A. K. MILLER** & **W. M. FURNISH**

(*State University of Iowa, Iowa City, U.S.A.*)

(Letter dated 18th September 1957)

In answer to your inquiry of last July 8, we wish to recommend affirmative action by the Congress. The Moore/Sylvester-Bradley proposal seems to be appropriate ; such a problem has existed for a century and has been given active attention for more than a score of years.

The greater proportion of paleontological research has involved fragments of unidentifiable " whole animals ". In addition, a considerable proportion of such research today involves discrete hard parts with demonstrated stratigraphic value. For example, tens of thousands of individual conodonts are being secured from single rock layers by methods which preclude the discovery of assemblages. So few aptychi have been found associated with their shells that they are inconsequential.

Taxa for " parts " have been and are being proposed irrespective of the implications. If the International Commission is to retain its stature as the regulatory body in nomenclature, it should take cognizance of this development and care for it officially.

Further consideration of this matter may necessitate an alteration in the specific proposal. The need can scarcely be denied.

DOCUMENT 1/33

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **G. UBAGHS**

(*Université de Liège, Belgium*)

(Letter dated 25th September 1957)

Je pense qu'il existe effectivement en Paléontologie Animale une situation rendant nécessiare de reconnaître des unités parataxonomiques analogues aux " form " taxa des paléobotanistes. Je soutiens donc volontiers la proposition faite par le Professeur R. C. Moore et Mr. P. C. Sylvester-Bradley.

Je crains toutefois que les difficultés d'application soient plus sérieuses qu'il ne paraît à première vue. Il me semble dès lors essentiel :

(1) de réserver à la Commission Internationale de Nomenclature Zoologique le soin de décider si tel groupe de fragments fossiles relève ou non des parataxa ;

(2) de considérer que des objets ressortissant normalement aux parataxa puissent quand même être attribués aux taxa s'ils présentent des caractères rendant leur détermination taxonomique raisonnablement certaine (comme, par exemple, les articles de tige de nombreux crinoïdes méso- et cénozoïques).

Je me suis pas compétent pour me prononcer sur l'opportunité de référer aux parataxa certains stades de développement d'organismes parasites récents.

DOCUMENT 1/34

Proposed amplification of the " Parataxa Plan " to cover the classification and nomenclature of " Zoogene Marks "

(Commission Reference : Z.N.(S.) 1056)

By **E. M. HERING**

(*Zoologisches Museum der Humboldt-Universität zu Berlin*)

(Enclosure to letter dated 15th October 1957)

I should like to support most enthusiastically and without reserve the proposal of R. C. Moore and P. C. Sylvester-Bradley for the recognition of the special category of " parataxa " for the classification and nomenclature of discrete parts or of life-stages of animals which are inadequate for identification of whole-animal taxa.

It seems to me to be desirable that the concept of " parataxa " should be amplified. The International Congress should direct that the classification and nomenclature of all traces which any animal which once existed has left behind should be in terms of " parataxa ". This does not only concern the different walking tracks, but also the often very characteristic feeding effects which are of importance in identification, e.g., in the case of insects. These may include all sorts of plant galls and leaf mines. In addition there should be included in the concept of " parataxa " the cases of case-bearing animals. Very often it is much easier to identify a species of *Coleophora* (Insecta : Lepidoptera) by the larval case than by the imago. Similarly in the case of an Agromyzid fly identification may best be made from the mine burrowed in the parenchyma of a leaf, and in that of a Cynipid Wasp of Gall Midge from the gall produced on any part of a plant. A considerable number of specific names were based for the first time only on such marks as plant galls and leaf mines, and the imagenes bred in later times were identifiable from the descriptions of their feeding marks.

Having regard to the very great importance of these marks of the above types for the identification of animals I propose a ruling by the International Congress directing that the classification and nomenclature of all types of " zoogene marks " be in terms of " parataxa ".

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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CASE No. 4

ARTICLE 4 : PROPOSED ADDITION TO COPENHAGEN DECISION 54(1)(b) OF A PROVISION RELATING TO THE METHOD TO BE FOLLOWED IN CITING THE DATE AND AUTHORSHIP TO BE ATTRIBUTED TO A SUBSTITUTE FAMILY-GROUP NAME PUBLISHED WHEN THE NAME OF THE TYPE GENUS OF AN OLDER NOMINAL FAMILY-GROUP TAXON IS REJECTED AS A JUNIOR SYNONYM OR AS A JUNIOR HOMONYM OF ANOTHER GENERIC NAME

PURCHASED
17 JAN 1958

(Commission Reference : Z.N.(S.) 1270)

NATURAL HISTORY

DOCUMENT 4/1

Submission of a Draft Text of a provision relating to the method to be followed in citing the date and authorship to be attributed to a substitute family-group name

By **G. H. E. HOPKINS**

(*British Museum* (*Natural History*), *The Zoological Museum, Tring, Herts, England*)

(Enclosure to a letter dated 14th August 1957)

Although it has been decided (*Copenhagen Decisions on Zoological Nomenclature,* Decision 56) that " authors' names and dates of publication for family-group names need not be cited, except, where so desired, in catalogues or similar works for historical purposes, or in discussions of the relations between names in the family-group ", yet it is sometimes necessary to cite these particulars, and it is even arguable that, now that the principle of priority has been extended to cover such names, it is desirable that their date and authorship should be given. Decision 56 in no way prohibits this being done.

2. No difficulty arises as to the method of citation of the date and authorship of family-group names in most instances, but if it should be decided (as I have proposed in a paper submitted simultaneously with the present paper[1]) that it is more important to preserve the objective identity of a family-group taxon than to apply strict priority, then the question of the date and authorship to be attributed to the name of the taxon becomes of importance. It seems to me that the solution introducing fewest complications would be to call such replacement names *nomina mutata* and to cite for them both authorship and date of the original erection of the taxon and the authorship and date of the alteration in the name. I therefore propose the adoption of the following provision :—

> Where a family-group name is replaced by a substitute name by reason of the fact that it is based upon a generic name which has been rejected either (i) as a junior synonym (either objective or subjective) of an older generic name or (ii) as a junior homonym of an older such name, the substitute family-group name so published shall be attributed to the author by whom the rejected name was published, and shall be regarded as having been published on the date on which the rejected name was published, the citation of that author's name and the date on which the rejected family-group name was first published to be followed by (a) the name of the author by whom the replacement name was published and the date on which that name was published and (b) the words " *nom. mut. pro* " and the family-group name for which it was published as a substitute, the particulars under (a) and (b) to be cited in brackets (parentheses).
>
> Example : the family-group name SARCOPSYLLIDAE was published in 1880 for a taxon based on the genus *Sarcopsylla* Westwood, 1840, a junior synonym of *Tunga* Jarocki, 1838 ; the name TUNGIDAE was first published by C. Fox in 1925. The citation should be in the form : TUNGIDAE Taschenberg, 1880 (C. Fox, 1925, *nom. mut. pro* SARCOPSYLLIDAE Taschenberg, 1880).

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[1] See Document 3/2 on the London Agenda Paper (reproduced on pages 255 and 256 of the present volume).

CASE No. 5

APPLICATION OF THE PRIORITY PRINCIPLE TO FAMILY-GROUP NAMES

(Commission Reference : Z.N.(S.) 1251)

DOCUMENT 5/1

Experience gained since the Copenhagen Congress of 1953 in applying the priority principle to family-group names with recommendations designed to facilitate the application of that principle in this field

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

The decision by the Copenhagen Congress of 1953 formally to incorporate into the *Règles* a provision that the relative priority to be accorded to taxonomically competing family-group names, i.e. to the names of validly established nominal family-group taxa having, as their respective type genera, genera currently regarded by taxonomists as belonging to the same family-group taxon, did no more than put into written form a practice which had in general been followed by zoologists for many decades. Nevertheless, the codification of a customary procedure is apt to bring to light previously unsuspected problems and it is convenient in such a case to review the application of such a provision after it has been in operation for a number of years in order to determine in the light of experience whether in practice the provision in question has been working satisfactorily.

2. At the first stage the effect of the Copenhagen decision was to make it necessary to ascertain in any given case by whom, when, and where the family-group name concerned was first published as the name for a family-group taxon of any rank within the family-group category (i.e. as the name for a family, subfamily, tribe, etc.). Second, it is necessary to ascertain whether from the taxonomic point of view the currently accepted name is the name for the oldest nominal family-group taxon having as its type genus a genus currently regarded as belonging to the taxonomic unit in question. In other

words, it is necessary first to ascertain by whom, when, and where the generic name *X-us* Brown, 1777, was first validly taken as the base for a family-group name (X-IDAE, or X-INAE, etc.). Having ascertained, let us say, that the genus *X-us* Brown was first taken as the base for a family-group name by an author Smith in 1850, it is then necessary to find out from the literature whether any other genus, say *Y-a* Gault, 1810, which is currently regarded as belonging to the same family-group taxon as *X-us* Brown, was taken as the type genus of a family-group taxon before the genus *X-us* Brown was so taken by Smith in 1850. If it is found that there is no competing family-group name of older date based upon the genus *Y-a* Gault, then the currently accepted family-group name X-IDAE Smith, 1850, is under the Law of Priority the valid family-group name for the genus *X-us* Brown and for any other genera which taxonomists may subjectively regard as belonging to the same family-group taxon as *X-us* Brown. If, however, it is found that there is an older nomenclatorially available family-group name—say, Y-IDAE Green, 1849—which is subjectively applicable to the same family-group taxon as X-IDAE Smith, 1850, by reason of the fact that, in the opinion of workers in the group concerned, the genera which are the respective type genera of these nominal family-group taxa (i.e. in the above imaginary case, the genera *X-us* Brown and *Y-a* Gault) are referable to the same family-group taxon, then under the decision taken by the Copenhagen Congress the valid name for the family-group taxon concerned is Y-IDAE Green, 1849, and not X-IDAE Smith, 1850. As will be obvious, the practicability of applying the foregoing provisions depends upon the possibility of obtaining accurate information as to the dates when each of any pair of competing family-group names was first validly published.

3. When after the close of the Copenhagen Congress in 1953 I first had occasion, in conjunction with specialists who had submitted applications to the Commission, to assist in establishing by whom, and where a given family-group name was first published, the novelty of the task and the lack of works of reference comparable with those long available for tracing generic and specific names led me at first to wonder whether the labour involved in applying the priority principle to family-group names might not be disproportionately great and, whether, despite the obvious advantages offered by the adoption of that principle, it might nevertheless be necessary on practical grounds to consider the possibility of inviting the next International Congress of Zoology to examine the possibility of devising some system for regulating the relative status of family-group names which could be more easily operated. Greater experience gained in the years which have since elapsed has however not only confirmed me in the view that the Copenhagen Congress was well advised when it laid down that the relative status of family-group names should be regulated by the priority principle and, in addition, has led me to the conclusion that, granted the adoption of certain measures described later in the present paper, the application of that principle to this class of name, though inevitably presenting certain difficulties in the initial phase, does not offer any insuperable obstacles.

4. In the four-year period which has elapsed since the Copenhagen Congress some practical experience has been gained in the operation of the priority principle at the family-group-name level. The results so obtained are, I think, of interest as throwing new light on this question. During the above period about four hundred and fifty family-group names have been placed by the Commission either on the *Official List of Family-Group Names in Zoology* or on the *Official Index of Rejected and Invalid Family-Group Names in Zoology.* The number of names so accepted by the Commission as nomenclatorially available and taxonomically valid which have thus been placed on the *Official List* is just over two hundred. The proposals in regard to about half of these names arose in connection with cases currently submitted to the Commission in connection mainly with the names of the genera which are the type genera of the nominal family-group taxa concerned, while the remainder arose in the course of the review by the Commission of the family-group-name implications of cases which had previously been dealt with by it only at the generic name level. The foregoing sample, though not large, has the merit that it is made up of the names of taxa belonging to a wide range of Classes in the Animal Kingdom. In this sense the present sample may, I think, be regarded as being representative, for the placing of the names concerned on the *Official List* involved the investigation by specialists of the early literature of a large part of the Animal Kingdom.

5. The practical experience derived from the foregoing investigations justify the drawing of two important conclusions. First, the difficulties involved in finding where a given family-group name was first published are, in general, considerably less than might have been expected, being definitely less formidable than I myself supposed (paragraph 3 above) when in the period immediately following the Copenhagen Congress I first took part in attempts to ascertain the original references for family-group names. Second, it is clear that the question whether family-group names published in vernacular form should be treated as being acceptable for the purposes of the Law of Priority is, in some groups at least, of considerably greater importance than was believed by the Copenhagen Congress.

6. On the general question of the practicability of applying the priority principle to family-group names, it usually happens that after a little preliminary investigation one—or at most two or three—authors are found to have been responsible for the most important of the family-group names in the Class or Order concerned. It is usually found also that in the group in question there is at least one work of fairly early date in which the family-group names known to the author are listed. though usually without bibliographical references or even dates. In the case of names published up to about the middle of the XIXth century—that is, names published during the first century following the starting point of zoological nomenclature—valuable assistance may be obtained also from the *Nomenclator Zoologicus Index Universalis* of Agassiz which contains references to all the family-group names known to that author.

Finally, in quite a number of groups there are recent—or fairly recent—comprehensive catalogues containing particulars of names published for family-group taxa. While none of these sources can serve to build up a complete list of the family-group names published for any given group, taken in the aggregate, they go a long way in this direction. In every case, however, the results so obtained require to be supplemented by whatever means are open to the specialists in the groups in question. On balance, I am now firmly of the opinion that the task of establishing a system of family-group nomenclature based upon the principle of priority, though one which in any given case involves a considerable amount of work is thoroughly practicable and I consider therefore that the Copenhagen Congress acted wisely in applying the priority principle to the names for taxa of the family-group category.

7. The second point of general interest which emerges from the investigations carried out since 1953 in connection with individual cases submitted to the International Commission by specialists in many groups of the Animal Kingdom is the need for a relaxation of the rather rigid conditions imposed by the Copenhagen Congress in the matter of the acceptance as an available name of a family-group name based upon the name of the type genus of the taxon concerned when that family-group name is published in a vernacular form, i.e. with a vernacular termination. First, it must be noted that, as is now clear, important family-group taxa in some groups are currently treated as having been established by authors who, though the first to recognise the groups concerned as constituting separate family-group taxa, did not propose for those groups names in due Latin form, applying to them names based on the Latin names of the type genera but not themselves Latinised. Examples of this kind are provided by the names published in vernacular (German) form in the forties of the last century by Emmrich for family-group taxa in the Class Trilobita and by Koch for such taxa in the Class Arachnida. In some cases the rejection of such names would lead to a change in the concept represented by the nominal family-group taxon owing to the fact that some allied genus was taken by some other author as the type genus of a family-group taxon before the name originally published in the vernacular form described above was republished in due Latin dress. In other cases the rejection of such names would lead to the attribution of the name for the family-group taxon concerned to some later author who certainly did not regard himself as having done more than make use of a name previously validly established (as he believed) by the author by whom it had been published in vernacular form only. The loophole provided for dealing with such cases by the Copenhagen Congress (1953, *Copenhagen Decisions zool. Nomencl.* : 35–36, Decision 53(2) was limited to securing the acceptance of family-group names as from the date on which they were published in vernacular form only where " in the opinion of specialists in the groups concerned, it is specially desirable in the interests of stability and universality of nomenclature ". In the light of present experience I am of the opinion that these words are unduly restrictive and that they serve only to disturb nomenclatorial practice. I suggest that it would be of advantage if this provision were to be redrafted in such a way as to provide that a family-group name based upon the name of the type genus but published with a vernacular termination is to be accepted as having acquired rights under the Law of Priority in virtue of having been so published in those cases where such a name is customarily so treated by specialists in the group concerned and is not to be rejected without the prior authority of the Commission. Such a provision would not only secure the desired end but in addition would have the advantage that under it the onus of taking action would be placed (where it properly belongs) on the shoulders of those who desire to upset nomenclatorial stability rather than upon those who desire to promote it.

8. I may perhaps be permitted to supplement the experience of other specialists by referring to a survey which, in such spare time as I have had at my disposal during the last few years, I have myself made of the literature relating to my own speciality (the Rhopalocera) for the purpose of building up as complete as possible a list of the family-group names published for the butterflies. In general, my experience tallied closely with that of the specialists in other groups who since 1953 have co-operated with the Commission in tracing family-group names directly involved in applications submitted by them for decision. For example, I quickly found that Leach, Swainson and one or two other early authors were responsible for most of the family-group names of early date. Considerable assistance was obtained also from sources similar to those which had proved useful in the other investigations referred to above. Thus, there is in this group a work published in the late forties of the nineteenth century (Doubleday's *Genera of Diurnal Lepidoptera*), in which an attempt was made to list all family-group names so far published and this proved very valuable, even though no bibliographical references were cited for these names. However, once it was known that a given name had been published by a particular author, it was usually possible with a little patience to find the original reference without great difficulty. In addition, this group is fortunate in that it possesses for most of the families concerned a general catalogue of relatively recent date—Junk's *Lepidopterorum Catalogus*—in which also an attempt was made to enumerate all previously published family-group names. At this stage a systematic search was undertaken of the Insecta portion of the *Zoological Record* from the commencement of that work in 1863 down to the present time. This search brought to light a few names which I had not previously noted. On the whole, however, especially in the middle period the compilers did not note newly published family-group names. This was no doubt partly because such names were not then definitely subject to the Law of Priority and partly because until recent times authors publishing new family-group names very seldom added an indication to show that those names were new. The investigations described above sufficed to build up a long list of previously published family-group names but required to be supplemented by the separate search of books and papers known to myself as being likely sources both for family-group names that had been overlooked or for information showing that known family-group names were published in earlier works than those recorded in the course of the preliminary investigations described above. While in any list of this kind it is inevitable that there should be some omissions, the list compiled in the present case is, I believe reasonably complete. Certainly, it contains all known family-group names of ancient date. The point which is relevant in the present connection is that by the means described in the present paragraph it has been found possible to compile a list of family-group names with their references which can be relied upon as providing a safe working basis for the application of the priority principle to family-group names in this particular group.

9. It must be admitted that the task described above was laborious and time-consuming and that in any given group it would be a serious waste of time if such a survey had to be undertaken separately by every specialist who had occasion to find out what family-group names were already available in his speciality. To some extent this difficulty could be overcome by the publication in some specialist serial of the results of investigations of this kind. This would not however provide a complete solution for, amongst other things, it would throw no light on the question of possible homonymy between family-group names in the taxonomic group so investigated and names for taxa of this category in other groups of the Animal Kingdom. I am therefore led strongly to the conviction that what is urgently required is a comprehensive catalogue of the names of family-group taxa in the Animal Kingdom comparable in accuracy and scope with that compiled by Sherborn for the names of genera and species in his great *Index Animalium.* Discussions are already proceeding with the British Museum (Natural History), the International Trust for Zoological Nomenclature and other interested bodies with a view to devising a scheme which, by making possible the preparation of a catalogue on the foregoing lines, would provide workers in systematic zoology and palaeontology with the essential tool in this field which they at present lack. From this point of view it would be of great value if at its meeting in London next year the Fifteenth International Congress of Zoology were to adopt a Resolution strongly urging the carrying-through of the project described above.

10. The publication of an *Index of Family-Group Names in Zoology,* 1758–1958, on the lines discussed above would be an immense boon and would clear up the position, so far as all past names are concerned. It would be important, however, that every possible step should be taken to prevent the recurrence in the future of the state of confusion and uncertainty which exists today as regards family-group names already published. Here the International Congress of Zoology could give help by inserting in the *Règles* a *Recommandation* urging every author, when publishing a new family-group name, clearly to indicate that it is a new name by adding some such notation as " fam. nov.", " subfam. nov.", etc.

11. The conclusions which I have reached and the recommendations which I desire to put forward for consideration are as follows :—

(1) The application of the priority principle to family-group names decided upon by the Copenhagen Congress in 1953 offers certain difficulties in the initial phases but is perfectly practicable and, being the principle long recognised as that which should govern the relative status of generic names and names of lower rank, is the most suitable for the purpose (paragraphs 3–6).

(2) It has been found in some groups that the conditions laid down by the Copenhagen Congress under which alone the status of availability can be acquired by a family-group name which, though based upon the Latin name of the type genus, is not itself published in a fully Latinised form, are too restrictive and it is desirable in the interest of nomenclatorial stability that the existing conditions should be relaxed in such a way that the relevant provision in the *Règles* should provide that, subject to the termination employed being duly Latinised by later authors, such a name is to be accepted as possessing the status of availability in those cases where the name in question is customarily accepted by workers in the group concerned and, in cases of doubt is not to be rejected without the prior approval of the International Commission (paragraph 7).

(3) The difficulty which at present faces zoologists in applying the priority principle to family-group names arises from the fact that, contrary to the situation which has long existed in the case of names published for genera, species and taxa of lower category, there exists no comprehensive catalogue of the names already published for taxa belonging to the family-group. What is urgently required is a work enumerating the names so far published for family-group taxa comparable in scope and form with the late C. D. Sherborn's *Index Animalium*. Discussions are proceeding for the purpose of initiating the preparation of such a work. When available, that work will be an essential working tool for all specialists in systematic zoology and palaeontology, and it is recommended that the Fifteenth International Congress of Zoology, when it meets in London, should adopt a Resolution giving its support for the above project and stressing its value and importance (paragraph 9).

(4) It is recommended that the London Congress should insert in the *Règles* a provision urging any author, when publishing a new family-group name clearly to indicate that that name is a new name by adding some such notation as " fam. nov.", " subfam. nov.", etc.

DOCUMENT 5/2

Support for the priority principle to family-group names

(Commission Reference : Z.N.(S.) 1251)

By Members of the
Committee on Entomological Nomenclature,
Entomological Society of America

(Communicated by Robert L. Usinger, under cover of a letter dated 4th November 1954)

During the past year two questions were referred to the Committee. These concerned actions taken by the International Commission at Copenhagen and were raised by paleontologists. The questions are stated below :—

. . . .[1]

2. The second aspect of the Copenhagen decisions in this field in which difficulties have been found is in relation to the application of the principle of Priority to the older names in the family-group. This has arisen on each case where so far the question has come up of putting a name of this group on the *Official List of Family-Group Names in Zoology*, namely, (a) in codifying the decision in *Opinion* 140 regarding the family name MEROPIDAE in birds, and (b) in the recent decision regarding the name *Acmaea* and family-group name ACHMAEIDAE (Gastropoda). In the first case it has been impossible so far to run down the place where the family-group name was originally published, in the second case, Cox found that it took two and a half days' work of his assistant and that even so the result could not be regarded as certain. All the specialists who have taken part in these searches have expressed the view that the search for the place of first publication of these names (for which nothing like the *Index Animalium* exists) is altogether too laborious and that some method other than strict priority ought to be devised for names of this group.

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[1] The first portion of this Resolution dealt with a different subject (the proposed maintenance of Copenhagen Decision 54(1)(a)), the text of which has been published as Document 2/12 (: 138 of the present volume).

The E.S.A. Committee voted on upholding the Copenhagen Decisions as follows :—

. . . .

Question 2 : 6 for ; 0 against.

CASE No. 6

ARTICLE 19 (COPENHAGEN DECISION 75) RELATING TO THE QUESTION OF THE CONDITIONS IN WHICH A SPELLING NOT SUBJECT TO EMENDATION UNDER COPENHAGEN DECISION 71 MAY BE REJECTED IN PLACE OF A SPELLING IN GENERAL USE

(Commission Reference : Z.N.(S.) 1264)

DOCUMENT 6/1

Proposed adoption of provisions in substitution for Copenhagen Decision 75

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 19th September 1957)

Continuity or Priority of Spelling ?

A genus of wood-wasps was named *Orussus* by Latreille in 1796. This was bad transliteration from the Greek *ὀρύσσα*, and two years later Fabricius corrected it to *Oryssus*. For more than a century entomologists wrote *Oryssus*, and ORYSSIDAE, and the original mis-spelling was wholly forgotten. In 1911 the late S. A. Rohwer again noticed the original spelling and attempted to restore it. A few authors, conscientious in following the International Code, have followed him.

2. Every taxonomist knows of innumerable similar cases. The rule requiring us to restore the original spelling may be likened to an imagined law requiring each citizen to restore the earliest traceable spelling of his surname. But until 1953 under the provisions of the *Règles* the original spelling had to be restored. Then at Copenhagen a clumsy process was devised by which a

zoologist who felt that the revival of an old spelling was objectionable could obtain a decision after an as yet indeterminate period of time (cf. *Copenhagen Decisions on Zoological Nomenclature* : 45, Decision 75).

3. It is common sense not to disturb our zoological language by revising these long-forgotten spellings. A stable set of names is an objective specified in the preamble of the *Règles*, but by some persons this is not interpreted to extend to spelling. Even the right of the Commission to vary the spelling under its Plenary Powers has been challenged (cf. E. G. Munroe, *Bull. zool. Nomencl.* **12** : 304), for the Plenary Powers were instituted in the first instance to prevent confusion arising from name-changing. It would be difficult to argue that restoration of the original spelling would be a cause of confusion. It is rather an irritation and inconvenience.

4. Since their inception at Monaco in 1913, the Plenary Powers of the Commission have been broadened. Their general purpose is expressed in the preamble to the *Règles* adopted at Copenhagen in these words : " It is also a primary purpose of the *Règles* to insure that . . . names shall be both stable and universally accepted. Where either of these objects is threatened by the application of any part of these *Règles* in an individual case, the Plenary Powers of the International Commission on Zoological Nomenclature afford relief." More specifically, the actual wording of the pertinent part of the Monaco resolution, as amended by the Copenhagen Congress is : " Plenary power is herewith conferred upon the International Commission on Zoological Nomenclature, acting for the Congress, to suspend the Rules as applied to any given case for the purpose of preventing confusion and of promoting a stable and universally accepted nomenclature ". This provision, especially as interpreted in the preamble, clearly covers a right to preserve a stability in spelling. Every attempt at restoration of a forgotten spelling promotes diversity of usage instead of universality, because many zoologists always fail to change over. In any event the Plenary Powers have been and are being invoked to conserve well established spelling of names, *cf.* C. J. Stubblefield " Validation under the Plenary Powers of the emendation to '*Peltura*' of '*Peltoura*' Milne Edwards " (*Bull. zool. Nomencl.* **12** : 252).

5. Secretary Hemming writes me that he is fully convinced that the procedure decided upon at Copenhagen, with its indeterminate delay, " clearly cannot produce a satisfactory result from the point of view of any zoologist who wishes (to know) within a reasonable space of time what the correct spelling of a given zoological name is, whether the emendation long in use or the original long-forgotten, incorrect original spelling ". Whenever the Commission places the name of a taxon on an *Official List*, it must consider all spellings that have been used for it, and cannot come to a decision under the Copenhagen plan until after the lapse of some time. Name-changing, in such cases, has been avoided during the past four years by resource to the use of the Plenary Powers.

But again Secretary Hemming says (*in litt.*) " It has always seemed to me that this is an unnecessarily heavy-handed procedure in a matter of this sort."

6. It seems to the writer that there is clear need for the Commission to be given power to decide upon the spelling that shall be used for any name that it is about to place upon an *Official List*, and that this should be an ordinary, not a plenary power. In case of a name currently spelled in different ways, six months' notice should be required before a decision is taken, in order that interested zoologists should have opportunity to express their opinions.

7. If this were done, the Copenhagen provision covering the matter (*Copenhagen Decisions* : 45, Decision 75) might best be rescinded, since any one desiring to conserve the usual spelling of the name of a taxon could obtain an earlier decision by applying to the Commission to have the name placed on an *Official List*.

8. For these reasons I wish to suggest that the following proposals be approved at the London Congress :—

(a) That the following amendment to the *Règles* be adopted : " In placing the name of a taxon on an *Official List* the Commission shall have power to adopt the most widely used spelling, even though it be not the original spelling. If two or more spellings are in current use, this shall not be done until six months after the proposal has been published in the *Bulletin of Zoological Nomenclature* ".

(b) Such action, when taken, shall not be deemed to be under the Plenary Powers, and shall require merely a majority favourable vote. Nevertheless, the same public notice shall be required as though the action were to be taken under the Plenary Powers.

(This would fit into Article 11, Section 3 of the proposed redraft of the *Règles*. The power should also be cited in Article 29, Section 6.)

(c) That Decision 75 appearing on page 45 of the *Copenhagen Decisions on Zoological Nomenclature* be rescinded.

9. That if the action proposed in paragraph 8(c) above be not taken, then the following should be added after paragraph (ii) of Article 11, Section 3(b) of my proposed redraft of the *Règles* : " (iii) Maintain the proposed usage pending final decision ". (The Copenhagen Decision made the proposal tentatively effective immediately, but my redraft is defective in not making this apparent.)

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[1] See Vol. **14** : 89.

CASE No. 7

ARTICLE 4 (COPENHAGEN DECISION 50(1)(a) and (b)) RELATING TO THE FORMATION OF FAMILY-GROUP NAMES

(Commission Reference : Z.N.(S.) 1265)

DOCUMENT 7/1

Proposed substitution of Revised Provisions in place of Copenhagen Decision 50(1)(a) and (b)

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 19th September 1957)

Continuity of Usage in the Spelling of the Names of Taxa of the Family-Group

In a paper submitted simultaneously with the present paper[1] a simplified procedure has been proposed for dealing with names of taxa currently spelled in an invalidly emended form. The same problem arises in connection with names of the Family-Group, which have come into current usage in a form that, although employing the correct suffix, is different from that specified in the *Copenhagen Decisions on Zoological Nomenclature* : 34, Decision 50(1)(a) and (b). I wish now to propose that the same procedure be extended to cover such names. I suggest that the following amendment be made to Decision 50(1)(a) referred to above.

(a) **Conservation of currently used spellings.** In placing the name of a taxon of the Family-Group on the *Official List of Family-group Names in Zoology* the Commission shall have power to adopt the most widely used spelling, if based on the name of the type-genus.

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[1] The document here referred to has been reproduced as Document 6/1 on the Agenda Paper (: 196–198 of the present volume).

While the appropriate suffix must be employed, the rule for formation of the name may be varied. Example : PIERIDAE instead of PIERIDIDAE[2], based on *Pieris*, genitive *Pieridis*.

(b) Such action, when taken, shall not be deemed to be under the Plenary Powers, and shall require merely a majority favourable vote. Nevertheless the same public notice shall be required as though the action were to be taken under the Plenary Powers.

2. This amendment could be fitted into the proposed redraft of the *Règles* as Article 28, Section 9(d).[3]

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[2] The particular case cited by Professor Bradley has recently been dealt with by the International Commission under its Plenary Powers. Under the decision so taken the spelling PIERIDAE was validated as against the technically correct spelling PIERIDIDAE. This decision has since been embodied in *Opinion* 500 (now in the press).

[3] See Vol. **14** : 205.

CASE No. 8

ARTICLE 4 (COPENHAGEN DECISION 45) (FAMILY-GROUP NAMES) : RELATIVE MERITS OF CONTINUITY AND PRIORITY RESPECTIVELY

(Commission Reference : Z.N.(S.) 1271)

DOCUMENT 8/1

Continuity of Usage or Priority in the Case of Names of the Family-Group

By J. CHESTER BRADLEY

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 19th September 1957)

A situation somewhat similar to that of the resuscitation of long-forgotten original spelling arises in connection with family names. I refer to instances in which a forgotten family name is discovered to have priority over the name currently and usually used. *Copenhagen Decisions on Nomenclature* : 33, Decision 45, set up a somewhat clumsy and long protracted procedure by which a decision in such a case can be reached. The application by Mr. Arthur N. Dusenbury, Jr. (1957, *Bull. zool. Nomencl.* **13** : 194–199), requesting the Commission to use its Plenary Powers to direct that the Family-Group name ORBULINIDAE, which has priority, shall not be given precedence over GLOBIGERINIDAE, which is of later date, illustrates the fact that authors, at least in some cases, are not willing to go through the procedure demanded by the Copenhagen provision, but prefer a more immediate decision by seeking action under the Plenary Powers.

2. As in the case of an accepted emendation, the Commission should have ordinary (not Plenary) power to settle such a case.

3. I say not Plenary Power because setting aside of the Rules is not involved. Since the Rules provide that priority in family names is not to be maintained in case of conflict with current usage, the Commission is merely called upon to enforce the rule by decision as to whether a given case comes under it.

4. I therefore propose that at the London Congress, Decision 45 of the *Copenhagen Decisions* be rescinded and replaced by the following :—

(a) The Law of Priority shall govern the names of taxa of the Family-Group, except that, in cases where priority is in conflict with general usage, the latter is to be maintained.

(b) In case a taxonomist observes that a family-group name having priority is in conflict with general usage, he shall request the Commission to decide what name is to be used, and to place it on the *Official List of Family-Group Names in Zoology.*

(c) In placing a family-group name on the *Official List of Family-Group Names in Zoology,* in a case where names based on different taxonomic genera have been proposed for that group, the Commission shall adopt the name that it believes will best serve continuity and universality of usage, even though that name does not have priority.

DOCUMENT 8/2

On the importance of maintaining continuity of usage at the family-group-name level

(Commission Reference : Z.N.(S.) 1271)

By **FRANCIS HEMMING**, C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 5th November 1957)

I desire strongly to support the proposal submitted by Professor J. Chester Bradley (Document 8/1) that means should be found for meeting the general wish of zoologists that established family-group names should not be liable to rejection as the result of the discovery of an older name given to a family-group taxon having as its type genus a genus subjectively regarded by taxonomists as referable to the nominal family-group taxon currently recognised.

2. I have given my reasons elsewhere (Document 5/1) for believing that the Copenhagen Congress was well advised when it decided to insert in the *Règles* a provision expressly applying the priority principle to family-group names. I agree therefore with Professor Bradley that the mechanism now to be sought must be in the nature of a provision which will provide an escape from difficulties in cases where priority is manifestly at variance with established usage. As Professor Bradley suggests, a provision for the settlement of such cases by a simple reference to the Commission would afford the most practical solution.

3. I have, however, two points on the nature of the mechanism to be adopted which I suggest require further consideration :—

(i) As in the case of generic names, so also in the case of family-group names, there is always room for differences of opinion among specialists on taxonomic grounds as to the zoological scope of the taxon so named, that is, as to the number, in the former case of species, and, in the latter case, of genera, which from the taxonomic point of view can properly be regarded as belonging to the unit (whether genus or family-group taxon) concerned. Express recognition of this consideration was given by the Thirteenth International Congress of Zoology, Paris, 1948 in relation to generic names when it directed that, in order to avoid the appearance of judging a purely taxonomic question in cases where there are differences of opinion among specialists as to the scope of the genera involved in any given case, the Commission when placing on the *Official List of Generic Names in Zoology*, the oldest of the generic names concerned, should also place on that *List* one or more of the later names with a note that these latter were placed on that *List* for use by specialists who consider on taxonomic grounds that the type species of those genera were not congeneric with the type species of the oldest established of the nominal genera in question (1950, *Bull. zool. Nomencl.* **4** : 237, 268). A similar principle has been applied by the Commission in recent years when placing family-group names on the *Official List of Family-Group Names in Zoology*.

In these circumstances it would not be sufficient, as suggested in Document 8/1, merely to prescribe that a family-group name which is not the oldest such name subjectively available for the taxon concerned should be placed on the *Official List*, for that action, though necessary, would not by itself provide the name in question with the necessary degree of security. What is required therefore is that when it approves an application for the protection of an established family-group name as against senior subjective synonyms, the Commission should direct that the entry of that name then to be made on the *Official List* should be endorsed with a direction that the name in question is not to be rejected on the ground that it is a junior subjective synonym of some other name.

(ii) It is, I think, desirable that an adequate opportunity for study and comment should be provided to zoologists in any given case before a decision is taken by the Commission under the procedure envisaged in Document 8/1. I accordingly recommend that it should be provided that every such application be published in the *Bulletin of Zoological Nomenclature* and that no decision should be taken by the Commission thereon until after the expiry of the six month period following the publication in the *Bulletin of Zoological Nomenclature* of the application concerned.

CASE No. 9

PROPOSED ADOPTION OF PROVISIONS REGULATING THE CITATION OF A NAME WHICH HAS BEEN EITHER EMENDED OR CORRECTED UNDER THE " RÈGLES "

(Commission Reference : Z.N.(S.) 1269)

DOCUMENT 9/1

Citation of the author of a corrected name

By J. CHESTER BRADLEY

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 19th September 1957)

The *Règles* require that in certain circumstances a name is to be automatically corrected but attributed to the original author and date. When the correction is first made, the bibliographical facts will be clear, but when some later taxonomist refers the corrected form to the original author and date (without explanation) it is likely to be misleading. A reader, looking up the original author and finding a different spelling or form of the word used, is apt to conclude that the citation is a misprint or *lapsus calami*. The citation of the name of the zoologist who establishes the name of a taxon is a matter of bibliographical record, not done as an honour to the author, and like any bibliographic record must be accurate.

2. It is apparent that there should be some provision in the *Règles* to avoid this confusion. I am not certain just how this may best be done, but in order that taxonomists may be thinking about the matter, I suggest that the following addition to the *Règles* be approved at the London Colloquium :

> When it is desired to cite the original author following a name that has been validly emended or been automatically corrected, but that under some provision of the code is to be attributed to its original author and

date, the author's name shall be followed by a comma and the adjective "*emendatus, -a, -um*", or the abbreviation "*em.*", to indicate that the name is being given in an amended or corrected form.

[This amendment would fit into the proposed revised text of the *Règles*, as paragraph 5(e) of Article 22[1]].

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[1] See Vol. **14** : 163.

DOCUMENT 9/2

Method to be followed in citing a name after its spelling has been emended or corrected

(Commission Reference : Z.N.(S.) 1269)

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 8th November 1957)

In Document 9/1 Professor Chester Bradley has raised the question whether it is desirable that provision should be made in the *Règles* for the adoption of some method for the citation of a name in cases where the original spelling of that name has been either emended or corrected under the *Règles*. Professor Bradley has tentatively put forward the suggestion that the *Règles* should provide in such a case either for the addition after the author's name of the adjective " emendatus " (cited in the appropriate gender) or for the use of the abbreviation " em." and has invited zoologists to comment on the issue so raised.

2. The foregoing question has arisen on many occasions in a rather special form when the International Commission has had under consideration applications for the addition of names (both generic and specific) to the *Official Lists*. On these occasions the Commission has taken the view that in the entry to be made on the *Official List* the name should be entered in its valid form, i.e. in its emended or corrected form, but that in addition the original incorrect spelling should also be cited. The principal reason for adopting this double method of citation has been to make it clear in the actual entry that the Commission, before placing the name on the *Official List*, had duly considered the relative merits of the Original Spelling on the one hand and on the other hand of the emended or corrected spelling, this being necessary in this context in order to show expressly in those cases where a spelling different from the Original Spelling has been adopted that this spelling has been adopted deliberately and does not owe its insertion on the *List* to inadvertence.

3. The method adopted for the foregoing purpose in entries made on the *Official Lists* has been, first, to cite the name in question in its valid spelling and second, immediately after the name so given, to add in brackets (parentheses) the original spelling preceded by the words " emend. of ". The following are examples of actual entries of this sort which have been made on the *Official Lists* :—

(a) *An entry made on the "Official List of Generic Names in Zoology" by the Ruling given in "Opinion"* 391 :

Muensteroceras (emend. of *Munsteroceras*) Hyatt, 1884 (Class Cephalopoda, Order Ammonoidea)

(b) *An entry made on the "Official List of Specific Names in Zoology" by the Ruling given in "Opinion"* 411 :

percnopterus (emend. of *perenopterus*) Linnaeus, 1758, as published in the combination *Vultur perenopterus* (Class Aves)

4. While for the special reasons explained in paragraph 2 above the adoption of a special notation to show that a name has been either emended or corrected is necessary in the case of an entry made on any of the *Official Lists*, I do not myself consider that this is necessary when in an ordinary book or paper a name is cited for taxonomic, as contrasted with, nomenclatorial purposes. It is certainly important that in a systematic work the attention of readers should be drawn in cases of this kind to the fact that the spelling adopted for a given name differs from that originally published, but it seems to me that the best way of doing this by the inclusion at least once in the work concerned of the full original reference for the name in question, a procedure which was originally recommended to zoologists by the Tenth International Congress of Zoology, Budapest, 1927, and which was incorporated into the *Règles* as a *Recommandation* by the Thirteenth International Congress of Zoology, Paris, 1948, when it reviewed the provisions of *Declaration* 7, in which in the meantime the Budapest Resolution had been embodied (see 1950, *Bull. zool. Nomencl.* **4** : 170). Beside giving the basic information as to where any given name was published, the adoption of this procedure has important advantages in the case of specific names by, for example, giving information as to the generic name in combination with which the specific name in question was originally published and, in the case of many adjectival specific names, information as to the gender adopted for the generic name concerned by the author of the specific name. This class of information cannot all be indicated by the insertion of formulae in the form to be adopted in the citation of a name and there does not appear to be any valid reason for picking out one of these items (the original spelling of an emended or corrected name) for special treatment.

5. If it were to be decided that some provision on this subject should be included in the revised text of the *Règles*, it is important that the provision

concerned should take the form of a *Recommandation* only, for to include it as a mandatory provision would offend against the Canon originally propounded by Dr. J. Brookes Knight of the Smithsonian Institution, Washington, D.C., that provisions of the present type should not be given mandatory status in the *Règles*, any contravention of this Canon involving an undesirable act of " ritualism ".

CASE No. 10

THE QUESTION OF THE LANGUAGE OR LANGUAGES TO BE ADOPTED BY THE FIFTEENTH INTERNATIONAL CONGRESS OF ZOOLOGY, LONDON, 1958, AS THE SUBSTANTIVE LANGUAGE OR LANGUAGES FOR THE REVISED EDITION OF THE "RÈGLES INTERNATIONALES DE LA NOMENCLATURE ZOOLOGIQUE" (INTERNATIONAL CODE OF ZOOLOGICAL NOMENCLATURE)

(Commission Reference : Z.N.(S.) 1259)

DOCUMENT 10/1

Proposal relating to the Substantive Text of the International Code of Zoological Nomenclature

By **CURTIS W. SABROSKY**

(*Entomology Research Division, Agricultural Research Service, U.S. Department of Agriculture, Washington, D.C., U.S.A.*)

(Enclosure to a letter dated 17th October 1957)

The original International Rules of Zoological Nomenclature first appeared in three approved versions, English, French, and German, with the French designated as the substantive text. The draft of the new Code, however, is in English, that being the native language of Professor J. Chester Bradley, who was charged by the Copenhagen Congress with preparing it. This English draft is the one to be scrutinised in the months before the London Congress and in the Colloquium and the Congress itself. The utmost effort will be directed toward making this text clear, accurate, and unambiguous.

2. Once that is done and approved, the French version which will then be prepared will of necessity be a translation of the English text. It would be rather unrealistic and anachronistic to declare that the translation was the

substantive text, because this would, in the event that some difference in meaning was later found to exist, validate and give pre-eminence to a translator's error which had never been approved by the Congress. Presumably—and ideally—there would be no differences in meaning, and both official texts should be considered to have equal force. If any differences are found later, or thought to exist, they should be referred to the International Commission for adjudication as to which shade of meaning was actually intended, or which is the most desirable.

3. In actual practice, a definitive text is rarely referred to by working zoologists, if a version of the Code exists in their own language. During the years that the French text has been the substantive one, the most widely used text was actually an unofficial English text published by the Biological Society of Washington and made available at a low cost. It would seem that all translations of the Congress-approved English text should be officially checked and approved by the International Commission, and that an appropriate statement of their approval should be published with each official translation, but that is probably an economically impracticable ideal.

4. **Proposal :** The proposal now submitted is :—

(1) That the Congress-approved English text and the official French translation of it shall have equal force ; and

(2) that any questions or suspicions of difference in meaning between texts shall be referred to the International Commission on Zoological Nomenclature.

DOCUMENT 10/2

Historical Survey of the question of the Language to be accepted for the Substantive Text of the " Règles Internationales " (International Code)

(Commission Reference : Z.N.(S.) 1259)

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature and Managing Director, International Trust for Zoological Nomenclature*)

(Note dated 8th November 1957)

In Document 10/1 Dr. Curtis W. Sabrosky has put forward a proposal for a change in the existing arrangements relating to the texts to be adopted as the Substantive Texts of the *Règles Internationales de la Nomenclature Zoologique* (International Code of Zoological Nomenclature). As those zoologists who were present at the Copenhagen Congress will recall, this question then gave rise to delicate problems which were only adjusted with considerable difficulty. It is evident therefore that particular care will need to be observed when this matter comes to be discussed at the London Congress next year if general agreement is to be secured.

2. In the circumstances it will, I think, be for the convenience of the members of the London Congress to have before them the following brief historical survey of this problem. The salient points are as follows :—

(1) The present *Règles* were adopted by the Fifth International Congress of Zoology, Berlin, 1901. The task of editing the document so approved was considerable and had to be postponed until it was possible for the Editorial Committee appointed to hold a series of meetings. It was not until three years after the Berlin Congress that the Committee was able to discharge the duty entrusted to it, the Committee holding a series of meetings for this purpose at Berne after the close of the meeting in that city of the Sixth International Congress of Zoology. The *Règles* so approved by the Editorial Committee were published officially in Paris in 1905.

(2) By a decision of the Berlin Congress the foregoing document was drawn up in three languages, namely : French, English and German. By a decision previously taken by the Fourth International Congress of Zoology, Cambridge, 1897, which was endorsed by the Berlin Congress, it was provided that the substantive text should be the French text[1].

(3) In the period following the official publication in 1905 of the Substantive French Text of the accompanying English and German Translations the arrangements described above worked with remarkable smoothness. In a few cases, however, definite discrepancies between the Substantive French text and the English Translation—I cannot speak for the German Translation—came to light and were a cause of difficulty.

(4) The question of the arrangements to be made in regard to the language of the Substantive Text was reviewed in Paris in 1948 by the Thirteenth International Congress of Zoology, by which a large number of additions to, and modifications of, the *Règles* had been adopted. As a result, the Paris Congress re-affirmed the decisions taken by its predecessors that the Substantive Text should be the French Text and at the same time instructions for the preparation of a French text embodying the decisions taken by that Congress, " together with a literal translation thereof in the English language " (1950, *Bull. zool. Nomencl.* **4** : 77, Minute 12(1)(a) ; 1950, *ibid.* **5** : 131, 148–149).

(5) The question of the language to be accepted as the language for the substantive test of the *Règles* was considered again by the Fourteenth International Congress of Zoology at its Meeting held at Copenhagen in 1953. When this subject was brought before the Section on Nomenclature of that Congress, Dr. Karl P. Schmidt, the Chairman of the Meeting, ruled that the vote first to be taken on this subject be confined to a vote by those zoologists who were not nationals of French-speaking or English-speaking countries. This resulted in an unanimous vote in favour of the substitution of the English language for the French language as the substantive language of the *Règles* (Code) as revised by the Paris and Copenhagen Congresses. The Section on Nomenclature as a whole then took a vote on this subject in which it decided that, subject to further discussion with the French zoologists, the Copenhagen Congress should be invited

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[1] " Ces Règles qu'on trouvera ci-après sont redigées en trois langues, conformement aux resolutions des Congrès, la version française faisant foi en cas d'incertitude, ainsi qu'en a décidé le Congrès de Cambridge " (Blanchard (R.), 1905, *Internationales de la Règles Nomenclature Zoologique* : 11).

at its Final Plenary Session to make the foregoing change in the substantive language of the *Règles* (Code). At the same time it was agreed " that in view of its importance, this subject should be separately reported to the *Comité Permanent* of the International Congresses of Zoology and that the suggestion for a change in the language of the substantive text of the *Règles* should be proceeded with only if the *Comité Permanent* concurred in that course " (1953, *Copenhagen Decisions zool. Nomencl.* : 99).

(6) Just before the opening of the Final Plenary Session of the Copenhagen Congress, Professor R. Spärck, President of the Congress, informed me that strong objection to the suggested change in the substantive language of the *Règles* had been taken by the French members of the *Comité Permanent* and that he feared in consequence that serious difficulties would arise if the proposal were to be placed before the Plenary Session then on the point of opening. In these circumstances Professor Spärck and I agreed that it would be inexpedient that this proposal should be placed before the Plenary Session, and when at that Session I presented the remainder of the recommendations agreed upon the the Section on Nomenclature, I made it clear that those recommendations did not include any proposal for changing the language of the substantive text of the *Règles* (Spärck, 1953, in *Copenhagen Decisions zool. Nomencl.* : 126).

(7) After the close of the Copenhagen Congress, I received from zoologists in a number of countries expressions of keen regret that the proposal for the change from French to English as the substantive language of the *Règles* (Code) had not been placed before the Final Plenary Session of the Copenhagen Congress. This led to correspondence between Professor Spärck and myself which culminated in November 1953 with an exchange of documents setting out a settlement which we had agreed between us. The first of these documents was a letter dated 3rd November 1953 addressed to Professor Spärck by myself as Managing Director of the International Trust for Zoological Nomenclature, the second, a Minute dated 7th November 1953 addressed by Professor Spärck, as President of the Congress to myself as Secretary to the International Commission. In this Minute, which Professor Spärck authorised and instructed me to include in the Official Record of the decisions on nomenclature taken by the Fourteenth International Congress of Zoology under his Presidency, he gave directions :—

" that, subject to the undertaking given by the International Trust for Zoological Nomenclature (in the letter dated 3rd November 1953 referred to above) that it will arrange for the preparation and publication of a French text of the revised Rules, identical in substance and in form with the English text, immediately after the English text has been finally approved :—

(1) The French and English texts of the International Rules of Zoological Nomenclature, as revised by the Thirteenth International Congress of Zoology, Paris, 1948, and as further revised and expanded by the Fourteenth International Congress of Zoology, Copenhagen, 1953, prepared in the manner agreed upon by the last-named Congress and published by the International Trust for Zoological Nomenclature are both authentic, that is, the French text is the substantive text of the Rules and so also is the English text.

(2) In the event of its having been alleged at any time that on a due construction of the words employed there is a difference of meaning in any provision as between the two substantive texts, the matter is at once to be laid before the International Commission on Zoological Nomenclature, whose decision shall be final ".

(1953, *Copenhagen Decisions zool. Nomencl.* : 128–131.)

(8) Following the exchange of documents between Professor Spärck and myself described above, the arrangement so arrived at was notified by myself, at Professor Spärck's request to the Société Zoologique de France, to whom the undertaking given in my letter to Professor Spärck of 3rd November 1953 was formally renewed. In acknowledging this communication, the Society very kindly undertook to make itself responsible for the preparation of the French text as soon as the English text was available. (Letter dated 27th January 1954 from Professor P. Vayssière, President of the Société Zoologique de France).

3. It will be noted that in general form the proposal submitted as Document 10/1 resembles the settlement laid down by Professor Spärck in his Minute of 7th November 1953 (paragraph 2(7) above) but that in that proposal Dr. Sabrosky recommends that, while the officially approved French text " shall have equal force " with " the Congress-approved English text ", it shall rank as " an official French translation " and not as " a substantive text ", the status which it would possess under the terms of Professor Spärck's Minute. It is this difference only which will require consideration by the London Congress.

CASE No. 1

(*continued from page* 184)

DOCUMENT 1/35

The "Parataxa Plan": Objection lodged by the Nomenclature Discussion Group, Washington, D.C., U.S.A.

(Commission Reference : Z.N.(S.) 1056)

(Copy of a letter with enclosure dated 13th September, 1957, from **CURTIS W. SABROSKY**, Chairman, and **ELLIS L. YOCHELSON**, Secretary.)

On behalf of the Nomenclature Discussion Group of Washington, D.C., we are submitting herewith the following material relevant to the Moore and Sylvester-Bradley application on the subject of Parataxa [Z.N.(S.) 1056] :

(1) Minutes of the Group's meeting of August 7, 1957, and

(2) Signed Voting Papers.

Dr. R. C. Moore's digest of the proposal for Parataxa was duplicated and distributed to the entire group (current membership, 113) the week before the meeting. The discussion meeting revealed overwhelming sentiment against the proposal, with most of the paleontologists expressing particularly strong opposition. Subsequently, the Minutes and a Voting Paper were circulated to the membership.

Votes were received from 27 paleontologists and 29 neontologists, total 56, all opposed to the proposal. One member, W. H. Hass, spoke at the meeting in favor of the proposal, but did not return a Voting Paper. We understand that he is writing you directly.

ANNEXE 1

Minutes of a meeting of the Nomenclature Discussion Group held on 7th August, 1957

A meeting of the Nomenclature Discussion Group of Washington, D.C., was held on August 7, 1957, to discuss Parataxa. Forty-one members of the group were present, approximately one-third being paleontologists. At the close of the meeting a vote showed virtual unanimity in opposition to Parataxa. The following paragraphs are to inform the I.C.Z.N. of the opinions of the group regarding this proposal, as requested in Secretary Hemming's letter of 8 July, 1957, and to show some of the reasons for the opposition to this proposal.

Opinions expressed both before and during the meeting indicate that opposition has three bases :

(1) Several workers representing such divergent fields of interest as ostracods, mollusks, fish and mammals expressed concern as to the effect of the recognition of Parataxa in their speciality. All agreed that significantly more confusion than clarity would result. Opposition was specially forceful in regard to provisions suggesting that Parataxa be used for immature stages. A specialist on parasitic nematodes expressed strong opposition and noted that the American Society of Parasitology is opposed to the proposal. Though it was not expressly mentioned, the general feeling was that if Parataxa were adopted for any group, no matter what safeguards were written into the rules, sooner or later attempts would be made to extend the concept of Parataxa to the taxonomy of many diverse groups.

(2) A second source of concern was with the underlying philosophy of the proposal. It is generally agreed that some poorly known taxa, and especially some based only on discrete fossil fragments, are of necessity more or less artificial. It is also generally agreed that one of the fundamental aims of taxonomy is to construct a natural classification of organisms, and that with increasing study of a group classification becomes more natural. The retroactive principle and the separation of Taxa and Parataxa are considered to be steps backward in any attempt to construct a natural classification. In essence, the proposal puts a premium, in some cases, on the naming of what are clearly artificial groupings rather than natural taxa.

(3) The third source of opposition and that most clearly expressed was that the proposal is unnecessary. Most members believe that a strict application of the Rules would solve most of the problems for which the Parataxa proposal was initiated. In those few cases where strict application of the rules would cause really significant confusion, recourse could be had to the Plenary Powers of the I.C.Z.N. to suspend the Rules. It was felt that the problems posed by conodont assemblages, for example, were not fundamentally different from those of other fields. They differ only in the degree of complexity, and no radical revision of the Rules seems warranted.

At the same time, it was recognised by many members of the group that students of conodonts are faced with a difficult problem. In view of the highly subjective nature of the interpretation of conodont assemblages and of subsequent identifications, it was suggested that synonymy of a conodont assemblage with previously named taxa, based on discrete conodonts, might be impossible to prove and should not be formalized.

In summary, the discussion group recognised that a problem exists in regard to the taxonomy of conodonts, but felt that the application of the Rules as they are presently written would solve the problem. They are almost wholeheartedly against the proposal for the establishment of Parataxa.

ANNEXE 2

Particulars of Voting Papers enclosed with the joint letter of the 13th September, 1957 from the Chairman and Secretary

(i) Votes by 27 palaeontologists who voted on the Parataxa Plan, with particulars of their respective specialities.

Name of specialist	Speciality of specialist	Whether " for " or " against " the " Parataxa Plan "	
		FOR	AGAINST
Applegate, S. P.	Fish	—	Against
Boardman, R. S.	Bryozoa	—	Against
Wythe Cooke, C.	Echinoids	—	Against
Cooper, G. A.	Brachiopods	—	Against
Douglass, R. C.	Foraminifera	—	Against
Duncan, Helen	Bryozoa and corals	—	Against
Dunkle, D. H.	Lower vertebrates	—	Against
Dutro, J. T., Jr.	Brachiopods	—	Against
Finks, R. M.	Porifera	—	Against
Grant, R. E.	Brachiopods Trilobites	—	Against
Henbest, L. G.	Foraminifera	—	Against
Kier, P. M.	Echinoderms	—	Against
Ladd, H. S.	Mollusks	—	Against
Nicol, D.	Pelecypoda	—	Against
Oliver, W. A., Jr.	Corals	—	Against
Palmer, Allison R.	Trilobites	—	Against
Reeside, J. B., Jr.	Ammonites	—	Against
Roberts, H. B.	Crustacea (Decapods)	—	Against
Sohl, N. F.	Gastropoda	—	Against
Sohn, I. G.	Ostracoda	—	Against
Todd, M. Ruth	Foraminifera	—	Against
Vaughn, P. P.	Amphibians and reptiles	—	Against
Wetmore, A.	Aves	—	Against
Whitmore, F. C., Jr.	Mammalia	—	Against
Wilson, D.	Mollusca	—	Against
Woodring, W. G.	Mollusks	—	Against
Yochelson, E.	Gastropods	—	Against

(ii) Votes by 29 neontologists who voted on the Parataxa Plan, with particulars of their respective specialities.

Name of specialist	Speciality of specialist	Whether " for " or " against " the " Parataxa Plan "	
		FOR	AGAINST
Anderson, W. H.	Insecta	—	Against
Andrews, J. S.[1]	Helminth Parasites	—	Against
Burks, B. D.	Insecta	—	Against
Cartwright, O. L.	Entomology	—	Against
Chace, F. A., Jr.	Crustacea	—	Against
Clarke, J. F. Gates	Insecta	—	Against
Crabill, R. E., Jr.	Myriapoda ; Arachnida	—	Against
Field, W. D.	Insecta ; Lepidoptera	—	Against
Gurney, A. B.	Insecta	—	Against
Handley, C. O., Jr.	Mammalia	—	Against
LaRue, G. R.	Trematoda ; Cestoda	—	Against
Lucker, J. T.	Nematoda	—	Against
McIntosh, A.	Zoo parasites of all kinds	—	Against
Morrison, J. P. E.	Mollusks	—	Against
O'Neill, Kellie	Insecta ; Thysanoptera	—	Against
Paradiso, J. L.	Mammalia	—	Against
Parfin, Sophy	Insecta	—	Against
Rehder, H. A.	Mollusca	—	Against
Sabrosky, C. W.	Insecta ; Diptera	—	Against
Sailer, R. I.	Insecta	—	Against
Schultz, L. P.	Fishes	—	Against
Schwartz, B.	Zoo parasites of all kinds	—	Against
Setzer, H. W.	Mammals	—	Against
Spilman, T. J.	Coleoptera	—	Against
Stone, A.	Insecta ; Diptera	—	Against
Taylor, W. R.	Fishes	—	Against
Todd, E. L.	Insecta	—	Against
Vogt, G. B.	Insecta	—	Against
Wirth, W. W.	Insecta ; Diptera	—	Against

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[1] In a letter dated 14th October, 1957, Dr. Andrews supplemented the vote here recorded as follows :

" I am taking this opportunity to express to you my full agreement with the conclusions of the Nomenclature Discussion Group of Washington, D.C., which on August 7, 1957, voiced its almost complete opposition to the proposal for the establishment of Parataxa.

" If Parataxa is adopted I should like to see the exclusion of the life stages of animal parasites which are now known only by Collective Group Names, because these forms are very often fully identified with recognised adult species at a later date ".

DOCUMENT 1/36

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **E. VOIGT**

(*Hamburg, Germany*)

(Letter dated 17th October 1957)

Ich bedaure, mich den Vorschlägen von Prof. Moore betr. " Parataxa " nicht anschleissen zu können. Die Einführung von " Parataxa " wird m.E. die Konsequenz haben, dass künftig nicht nur bei Conodonten oder Aptychen, sondern auch bei allen anderen, aus mehreren Skelettelementen bestehenden Fossilien eine gesonderte Benennung für die Einzelelemente gefordert werden wird. Eine Spezies kann aber nur einen einzigen gültigen Namen haben. Sobald die Zusammengehörigkeit von Einzel-Elementen erkannt ist, fallen sie automatisch als Synonyme weg.

Im Falle z.B. der Aptychen wäre m.E. zu fordern, dass das Ammonitengehäuse in jedem Falle vor dem Aptychus rangieren muss, d.h., wenn ein Aptychus vor der Schale formal die Priorität hat (z.B.*Trigonellites* vor *Oppelia*,) so ist nach der Auffindung der Aptychen in der Ammonitenschale ersterer als Teil der letzteren erkannt und müsste in die Synonymie von *Oppelia* fallen.

Es wäre daher zweckmässiger, eine Änderung des Artikels 27 dahingehend zu erwirken, dass Einzelteile, z.B.Aptychen, Opercula von Würmern oder Gastropoden, Otolithen von Fischen oder sonstige Einzelteile von Vertebraten ihren Genus-oder Speziesnamen verlieren, wenn sie trotz ihrer Priorität als *Teile eines bereits beschriebenen ganzen Organismus* erkannt werden. Sie erscheinen dann in her Synonymie der betreffenden Spezies. Es wäre festzulegen, welche Teile vor den anderen rangieren.

Letzten Endes sind die " Parataxa " nichts anderes als die bereits früher vorgeschlagenen " Allotypen ", welche sich ja auch nicht eingebürgert haben.

Ich kann also die Fragen 1 und 2 Ihres Briefes nur negativ beantworten.

DOCUMENT 1/37

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **LEIF STØRMER**
(*Paleontologisk Institutt, Oslo, Norway*)

(Letter dated 22nd October 1957)

In reply to your letter of 8th July with the proposed addition to the *Règles* of provisions recognising and regulating the nomenclature of " parataxa ", I wish to say that I have studied the edition and say " Yes " to the two groups of issues involved. (Satisfactory safeguards are among others given in 7(ii)(b).)

DOCUMENT 1/38

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **WILBERT H. HAAS**

(*U.S. National Museum, Washington* 25, *D.C., U.S.A.*)

(Letter dated 23rd October 1957)

I consider the proposal submitted by Dr. R. C. Moore and Mr. P. C. Sylvester-Bradley on that subject to be appropriate for the purpose it was devised to serve and to contain adequate provisions for protecting the interests of investigators concerned with the nomenclature of whole-animal taxa. Parataxa provide a means whereby investigators can better meet the continually developing taxonomic needs of certain groups of organisms, including conodonts—the group of microfossils in which I am especially interested.

I have been a student of conodonts for the past 20 years, chiefly because they are an extremely useful tool of the stratigraphic paleontologist. I am convinced that progress in the study of conodonts will be most rapid under a system that permits the classification and nomenclature of discrete conodonts to be in terms of Parataxa and the classification and nomenclature of conodont assemblages, to be in terms of whole-animal taxa. Such a system would provide students of conodonts with a stable nomenclature, which is something they do not have at present.

DOCUMENT 1/39

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **VLADIMÍR POKORNÝ**

(*Caroline University, Prague, Czechoslovakia*)

(Letter dated 10th October 1957)

I agree with the proposed insertion in the *Règles* of " parataxa ", because the stability of these very important fragments as well as of the true taxa related with these is highly desirable.

2. On the other hand I would like to emphasize the opinion of Prof. Chester Bradley that the parataxon " in a zoologically more important sense is outside of taxonomy " (Document 1/1, para. 5, p. 8). This opinion must be respected to the extreme.

3. The basic principle of every scientific classification is to exprime the phylogenetic relations. The nomenclature should help to this purpose as well as possible. Many fragments, the taxonomic place of which cannot be ascertained as to the specific category, can without difficulties be classified within natural genera or higher taxonomic groups. It is not advisable to obscure the result of the taxonomy of the fragments by a " nothing-saying " nomenclature. This has been well expressed already by Klaus J. Müller (1956) in discussing the same problem : " It is contrary to the basic aim of nomenclature that either observations or ideas on taxonomy are to be suppressed because there is no *proper* way for their expression " (Müller, *J. Pal.* 30(6) : 1328).

4. In the light of the above principles I make the following comments :

(1) The parataxa, an aid-tool, should be erected only in unavoidable cases.

(2) The names of fragments should be as significative as possible.

(3) In view of (2) above the new names should, when possible, point to the corresponding taxon (genus or higher category).

(4) It should be recommended that the names for parataxa should derive from the morphological terminology of fragments already in common use in paleontology and zoology.

5. In accordance with Proposition (1) above the aid-field of parataxa must rest on a field of serious science. Its conversion into a sort of philately would be highly undesirable. The newly opened field of parataxonomy could easily become a domain of pseudo-scientists the highest aim of which would lie in the " Namenmacherei " or " Mihismus ", as it was properly designated by R. Richter.

6. There would be no sense in erecting new names for fragments described previously as parts of a known taxon, e.g., a crinoid. Each carefully worked-out crinoidal paper could become a base for erecting hundreds of new parataxal names created by a mere indication by a " Philatelist " without a scientific background. Such a procedure must be considered as one which would be highly harmful to the literature and the scientist's mind.

7. In the recommendations following the new proposed paragraph it should be therefore strongly emphasised that the erection of parataxa must rest only on " emergency exit " for cases where the application of normal taxa is impossible.

8. Proposition (3) in paragraph 4 above concerns paragraph 11* of Document 1/1. It departs from the fact that the family—or generic place—of fragments is in many cases identifiable, but not the specific one. In my opinion it would be of great advantage if the name for such para-species were to indicate its family—or generic—position. This would also be very useful in work with parataxa.

9. Such a procedure has been in reality for many years in fact for many decades been in common use within otoliths, a group not mentioned in the Case No. 1, but one which can reveal further possibilities and difficulties not considered in the propositions published by the applicants. It is not the place here to argue why a parataxal classification of this group is needed, but I feel that the case is quite clear.

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* See page 11 of the present volume.

10. The common procedure of naming otoliths is as follows : The technical term " Otolithus " written with an initial capital is taken instead of a generic name. Behind it follows (though not always) in parenthesis the name of the family or of the genus to which the species belongs, for example :—

(a) Otolithus (Serranidarum) elongatus Weiler, 1942

(b) Otolithus (Scopelus) pulcher Procházka, 1893

(c) Otolithus (inc. sedis) dispar Koken, 1891

Considered from the standpoint of the *Règles* this commonly used practice is not faultless. Thus, in (b) above the name of the genus *Scopelus* must, under the application of the Rules, be considered as that of a sub-genus. On the other hand, considered purely from the objective side, this procedure is very clear and useful.

11. Further serious problems arise when considering the content of the " generic " name " Otolithus ". It is known that, contrary to the situation in the conodonts, the form of otolith is specific for each genus, so that on the basis of otoliths, form-genera may be erected which by their scope are equal to natural genera. Using the present practice a new para-genus could be designated, e.g. Otolithus (Boiolithus n. paragenus) typicus Charles. Under such a procedure the name Otolithus could not be considered as a para-genus, but being preferably regarded as a higher parataxon not defined as yet. Such a denomination is not desirable.

12. In view of the fact that the shape of the otoliths is specific for different genera, many authors designate the otoliths in full accordance with the *Règles* by the names of natural genera, e.g., Scopelus pulcher Procházka, 1893. The objections cited in paragraph 7 of Document 1/1 are valid against such a procedure.*

13. The simplest way for designating otoliths, the great majority of which can be attributed without difficulties to natural families and even genera, would be by means of a binomen in which the parataxon would be marked by a sign or abbreviation, e.g., pg. for paragenus, ps. for paraspecies. A quite similar proposal has already been made by K. J. Müller, 1956 (*J. Pal.* **30** : 1328) in discussing the nomenclature of conodont genera :—

(a) pg. Boiolithus typicus Charles, 1859 ; (b) Scopelus ps. pulcher Procházka, 1893. In the case of (a) it superfluous to write *ps.* Such a procedure makes it very easy to draw paleoecological, paleogeographical and other conclusions on the base of Ichthyofauna. It is convenient also for those specialists which are not especially engaged in the study of otoliths, as are many vertebrate paleontologists or zoologists.

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* See pages 9–10 of the present volume.

14. We may now summarise the views expressed above in regard to Proposition (3) as set out in paragraph 4 above. From the standpoint of securing the maximum possible correspondence between nomenclature and scientific results, as well as from the standpoint of clearness and simplicity in nomenclature I recommend to include the following provision in the suggested new paragraph of the *Règles* :—

(a) When the generic taxonomic position of a given fragment is known, the paraspecific name is combined with the generic name. When only the family or higher taxon is known, the family (or higher taxon) name is used in genitive form following the paragenus (para-subgenus) name.

(b) In the case of such heterogenic *nomina*, parataxa are designated by an abbreviation placed immediately in front of the parataxon name.

15. In my view Proposition (3) is of great practical importance. The object has already been met in the case of aptychi, where the genera have the significative ending—aptychus. A similar procedure represents the present practice in naming otoliths, as has been shown in paragraph 10 above.

16. Personally I should consider it as very desirable if such a procedure were to be recommended by the *Règles* for use also in other groups of parataxa that may be formed. As an especially suitable example I may cite the spiculae of sponges. For this group a very detailed and ingenious terminology was worked out as can be seen, e.g. in the compendium of H. Rauff (1893–1894), in his Palaeospongiologie (*Palaeontographica* **40**, **41**, Stuttgart). I am convinced that the conversion of these morphological terms into paragenera by merely writing them with initial capitals (oxyhexactin—Oxyhexactin ; criccaltrop-Criccaltrop ; dichotriaen—Dichotriaen) would be the best course that could be adopted. If so, the same terms would indicate the components of natural genera and the corresponding paragenera. Such a procedure would have also practical consequences, especially in the stratigraphical application of the parataxa, while it would facilitate work with parataxa to non-specialists as well as for specialists working in smaller laboratories.

Remarks on Document 1/1, paragraph 12 (: 12) and paragraph 13 (: 13)

17. The following comments are made on the under-mentioned passages Document 1/1 :—

(a) Paragraph 12(1) reads : " the parataxa system should be recognised as validly applied to those animal-groups specifically authorized for that purpose by the Commission ".

(b) Paragraph 13(2)(b)(i) however, runs as follows : " any zoologist desiring that classification and nomenclature of a particular group of animal fragments should be made in terms of parataxa . . . "

18. The phrase " animal groups " used in paragraph 12 is not appropriate as in this sense it has not always borne the same meaning as " animal fragments ", the phrase used in paragraph 13. This will be seen from the following example : Among the Pisces can be classified as " fragments " otoliths, teeth, scales, bones, but it does not seem advisable to classify all these fragments in terms of parataxa. Thus for example the great majority of taxa in Selachii is based on teeth exclusively. The new provisions must therefore be in terms of about " groups of animal fragments ".

DOCUMENT 1/40

Rejoinder to the objections to the " Parataxa Plan " advanced by the Nomenclature Discussion Group, Washington, D.C., U.S.A.[1]

(Commission Reference : Z.N.(S.) 1056)

By **P. C. SYLVESTER-BRADLEY**

(*University of Sheffield, Sheffield, England*)

(Enclosure to letter dated 1st November 1957)

I would like to preface my comments with a personal note. I am not myself a student of any of the groups for which parataxa are proposed. I have no specialised taxonomic interest in either conodonts or aptychi. My collaboration with R. C. Moore in the production of the " Parataxa Plan " was undertaken, therefore, not in the capacity of a specialist, but as one of the International Commissioners who is also a palaeontologist. My present interest, in this period before the London Colloquium, is therefore, not that of the parent of a fond child, but still that of one of the Commissioners who is attempting to find a solution to a difficult nomenclatural problem. As such, I am just as interested in clarifying the grounds for opposition as I am in accumulating constructive ideas for the improvement of the proposals. I hope, therefore, that some members of the Washington Group will be prepared to add weight to their arguments by dealing in more detail with some of the points noted below.

1. It is proper that the grounds for opposition should include statements of opinion, but arguments such as that which reads : " all agreed that significantly more confusion than clarity would result " would gain much in force if they were related to some of the specific cases dealt with in the Proposal. As it stands, para. 1 seems to express general misgiving, coupled with opposition to the adoption of the " Parataxa Plan " as a method of dealing with immature stages. A reasoned statement comparing the advantages and disadvantages of the " Parataxa Plan " with the Collective Group Plan as put forward by the American Society of Parasitologists (*Bull. zool. Nomencl.* **15** : 88–98) would be of much value during the London meetings. General opposition from workers in " ostracodes, mollusks, fish and mammals " seems to have no weight unless it is related to specific proposals in those groups. If a mollusk specialist can show how recognition of aptychi as parataxa can result in significantly more confusion than clarity, that would be of great importance. Similarly, it would be useful if a fish specialist could outline objections to recognising otoliths as parataxa, for though no proposal has yet been published suggesting such a course, it seems likely that some specialists in otoliths may feel the " Parataxa Plan " has value in their case. But nobody has suggested that the " Parataxa Plan " can usefully be applied to ostracodes or vertebrates ; if they did, opposition would be widespread.

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[1] See Document 1/35 (: 216–219 of the present volume).

2. Opposition to the underlying philosophy legalizing the present existence of dual nomenclatures in certain groups is sound, but please, if possible, couple this opposition with an alternative proposal of how to deal with the present situation in either or both conodonts or aptychi. One such alternative was proposed by Arkell (reprinted *Bull. zool. Nomencl.* **15** : 71–75) ; but Arkell himself now supports the " Parataxa Plan " (*Bull. zool. Nomencl.* **15** : 78). Please indicate whether this alternative is supported, and, if so, whether the names to be suppressed under the proposed *Declaration* " as established solely on aptychi " are to be suppressed for all purposes, or only for the purposes of the Law of Priority.

3. The " most clearly expressed course of opposition " is contradictory as at present worded. How can " strict application of the Rules " solve the problem, if at the same time the " synonymy of a conodont assemblage with previously named taxa, based on discrete conodonts . . . should not be formalized " ? If the synonymy is not formalized, Article 27 is ignored, and the dual nomenclature, which at present exists, continues with flagrant disregard to " strict application of the Rules ". The " Parataxa Plan " is one method of legalizing such a dual nomenclature. Another way might be to add a simple rider to Article 27, stating : " This Article cannot, however, be applied to the nomenclature of all fossil groups ". But it is illogical in the one paragraph to recommend strict application of the Rules, and in the next to advocate their disregard.

4. In conclusion may I express a hope that all alternative suggestions for dealing with the problems of dual nomenclature will fully recognize the principle, to be written into the Preamble to the new Rules, that the Rules must not trespass upon the freedom of taxonomic thought. The Rules are not the place for one group of taxonomists to curb the activities of another. Researchers investigating the taxonomy of conodonts or aptychi have as much right to make use of the Rules as have other taxonomists.

DOCUMENT 1/41

Comment on the " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **FRIEDRICH TRAUTH**

(*Vienna, Austria*)

(Enclosure to letter dated 28th August 1957)

Was zunaechst die allgemeine Bezeichnung der Verschlussdeckel (Opercula) der Ammoniten betrifft, so haben wir uns schon 1927 (Trauth 1927, p.221) dazu entschlossen, den dafuer 1829 von H. v. Meyer (Das Genus Aptychus. Nov. acta phys-med. Acad. caes. Leop. Carol. nat. cur. Vol. XV, pars II p. 125 [Berlin u. Bonn]) vorgeschlagenen und vorherrschend auch im palaeontologischen Schrifttum dafuer gebrauchten Namen "*Aptychus* " zu verwenden und nicht den eigentlich aelteren und ihnen — allerdings unter der irrtuemlichen Voraussetzung, dass sie Bivalven seien — von J. Parkinson (Organic remains of a former world. III Vol. p. 184, 186 [London]) gegebenen Ausdruck " *Trigonellites* " (vgl. Trauth 1927, p. 173, Fussnote 1. u. p. 221).

Die einschaligen und so gewissermassen der Verschmelzung der beiden Valven eines *Aptychus s. str.* entsprechenden *Anaptychi* (*Anaptychus* Opp.= Arten) sind wie wohl auch erstere fruehestens aus dem Devon bekannt geworden.

Wenn wir nur die in den einzelnen Formationen auftretenden Opercula naeher betrachten, so haben wir zunaechst aus dem Palaeozoikum (Devon, Karbon, Perm) und zwar dem von Deutschland, Frankreich, Belgien, Russland und der Vereinigten Staaten die Formen ("Arten ") des Typus (der " Gattung ") *Anaptychus* Opp. zu erwaehnen, welche von den Goniatiten (bes. von *Manticoceras Crickites* und eventuell auch von *Beloceras*) herstammen (1934*a*, p. 47, Taf. 2–3) und welche da gelegentlich wohl auch von (leider nicht genauer bestimmbaren) zweivalvigen *Aptychi* (1934*a*, p. 70, Taf. 3 Fig. 19) begleitet zu sein scheinen. Oder sollte es sich bei den letztgenannten Opercula etwa auch nur um einschalige, infolge ihres Erhaltungszustandes " zweischalige Aptychen " bloss vortaeuschende *Anaptychi* gehandelt haben ?

Die aus der Trias (speziell der Nord-und Suedalpen und auch Groenlands) bekannt gewordenen *Anaptychi* sind wohl namentlich auf die Ammonitengattungen *Ceratites* ?, *Glyptophiceras* ? *Protrachyceras* oder *Trachyceras* ?, *Monophyllites* (*Mojsvarites*) und *Arcestes*, resp. *Proarcestes* ? beziehbar (1935*a*, p. 456, Taf. I).

Im Lias von Deutschland, Frankreich und England sind *Anaptychi* von den Ammonitengenera *Psiloceras*, *Proarietites*, *Arietites* (mit den Subgenera *Arnioceras*, *Eparnioceras*, *Coroniceras*, *Vermiceras* ? und *Asteroceras* ?), *Aegoceras* ?, *Amaltheus* und *Lytoceras* veroeffentlicht worden (1934*b*, p. 70 ff. Taf. VI).

Waehrend aus dem Dogger und Malm — wenigstens vorlaeufig — noch keine *Anaptychi* bekannt geworden sind, sehen wir dieselben dann wieder in der Unter- und Oberkreide des westalpinen Neokoms (Freiburger Alpen und Suedostfrankreich) und in der Oberkreide Japans auftreten. Sie stammen hier wohl im Wesentlichen von dem Ammoniten-Genus *Lytoceras* (?), resp. der diesem zunaechst stehenden Gattung *Gaudryceras* ab (1935*c*, p. 448 ff., Taf. XIV). Und lytocerate Herkunft gilt so wohl auch fuer den *Anaptychus*-Subtypus *Sidetes* Gieb. mit seiner einzigen aus dem Senon des Quedlinburger Salzberges bekanntgewordenen Form *S. striatus* Gieb. (1935*c*, pp. 458–459, Taf. XIV Fig. 4).

Anknuepfend an die ebenerfolgte Eroerterung der Kreide-*Anaptychi* moege hier noch kurz zweier gleichfalls der Kreideformation (und zwar dem Senon) entstammender und durch ihre Einschaligkeit wohl sehr daran erinnernder Typen gedacht sein, deren je einzige Artvertreter aber durch eine gerade " symphysale " Mitellinie eher an die echten zweivalvigen Aptychen erinnern und auch aus solchen durch eine nachtraegliche Valvenverschmelzung hervorgegangen sein moegen. Wir meinen da einerseits den *Neoanaptychus semicostatus* Nag. von *Desmoceras* (*Kotoceras*) *semicostatum* Yabe des japanischen Senons (vgl. *Neoanaptychus semicistatus* Nag. bei Trauth, 1935*c*, pp. 459–462, Taf. XIV, Fig. 12–14) und anderseits *Pteraptychus numida* Coqu. (vgl. bei Trauth, 1935*c*, pp. 462–465, Taf. XIV Fig. 15) aus dem algerischen Senon, dessen Konvexflaeche durch die von jedem der zwei Lateralraender externrandwaerts geneigt der " Symphysen-Linie " zustrebenden Rippen gekennzeichnet erscheint ; die von uns frueher einmal geaeusserte Meinung, dass es sich dabei eventuell auch um ein Desmoceratiden-Operculum handeln koennte, hat jedoch seither keine Bestaetigung erfahren koennen.

Wir wenden uns nun der kurzen Eroerterung der zweivalvigen Aptychen der Juraformation und der Unterkreide zu, worauf wir dann schliesslich diese Arbeit mit der Kennzeichnung von deren Oberkreide-Formen beendigen wollen.

Wir beginnen mit einem weitverbreiteten Opercular-Typus, naemlich mit *CORNAPTYCHUS* Trauth (1927, pp. 214, 228, 236 ; 1930, pp. 345–354, Taf. III Fig. 4–16 ; 1935*d*, pp. 22–58, Taf. V–VI ; 1936*a*, pp. 10–28, 40–43, Taf. III Fig. 1–6), welcher in einem weiten Gebiete — in Sued-und Norddeutschland, Frankreich, England, den West-und Ostalpen, Karpathen, Sizilien und Japan — vom Mittellias bis zum obersten Dogger erscheinend, doch namentlich im Oberlias vorkommend, vorwaltend der Ammoniten-gruppe der *Harpoceratinae* entstammt und zwar den Gattungen *Harpoceras s. str.*, *Leioceras*, *Pseudoleioceras*, *Grammoceras*, *Pseudogrammoceras*, *Hildoceras* und *Hecticoceras*), ferner selten auch den *Hammatoceratinae*-Gattungen. *Acanthopleuroceras* (Cycloceras ?) und *Sonninia* und der *Polymorphinae*-Gattung *Dumortieria*.

Er besitzt eine haeutig-duenne kohlige Unterschicht und eine recht zarte (D : B < 0.06) und konvexseitig imbrikat berippte Kalkschale (Oberschicht), die aus bloss geringfuegige Zellraeumchen darbietenden und also ziemlich kompakten und sich parallel-imbrikat uebereinander legenden Zuwachsblaettern besteht (1935*d*, p. 29).

Und nun eine gedraengte Kennzeichnung der sonstigen jurassisch-unterkreidischen Aptychentypen :

LAEVICORNAPTYCHUS Trauth (1936*a*, pp. 28–36, Taf. III Fig. 7–10). Dieser im Oberlias von Wuertemberg und Nordfrankreich mit der Ammonitengattung *Harpoceras*, resp. deren Subgenus *Pseudoleioceras* und eventuell auch noch im Mitteldogger (oberem Bajocien oder Bathonien) der Freiburger Alpen (Schweiz) mit *Hecticoceras* oder *Oppelia* verknuepfte Opercular-Typus stimmt strukturell — durch den Besitz der haeutig-duennen, kohligen (urspruenglich wohl hornig-chitinoes gewesenen) Unterschicht und der auch sehr duenn bleibenden kalkigen Oberschichte der Schale — voellig mit *Cornaptychus* ueberein, und unterscheidet sich von ihm bloss durch die ganz oder doch ziemlich glatte und also hoechstens schwaechste konzentrische Rippenstreifen oder Runzeln und demnach keine markanten imbrikaten Rippen tragende Valvenkonvexflaeche.

Laevicornaptychus steht also seiner Schalenbeschaffenheit nach zu *Cornaptychus* etwa in demselben Verhaeltnis wie *Laevilamellaptychus* zu *Lamellaptychus* (vgl. p. 6 u. 7).

Von *Praestriaptychus* (vgl. nachstehend) unterscheidet er sich insbesondere durch seine ganz glatte oder doch merklich schwaecher konzentrisch gestreifte oder gerunzelte Valvenkonvexseite.

Praestriaptychus Trauth (1927, pp. 219, 230, 233, 241 ; 1930, pp. 378–387, Taf. V Fig. 11–18 ; 1937, pp. 135–152, Taf. 10 Fig. 1–12, Taf. 11 Fig. 1–4).

Ad *Stephanoceratidae* : ? *Stephanoceras* ?, *Sphaeroceras*, *Perisphinctes*, *Holcostephanus* ; ad *Cosmoceratidae* : *Cosmoceras*, *Parkinsonia*, *Kepplerites* ?, *Hoplites*.

Die Scheidung der gemeinsam bei den Ammonitenfamilien der *Stephanoceratidae* und *Cosmoceratidae* vorkommenden Opercula, die wir als die *Praestriaptychi* und *Granulaptychi* (vgl. p. 5) bezeichnet haben, auf Grund der bei den letzteren praevalierenden, bei den ersteren aber hinter der konzentrischen Runzelung zuruecktretenden konvexseitigen Granulationsskulptur ist — angesichts ihrer so gleichartigen Herstammung — sicherlich eine kuenstliche und in gewissem Grade willkuerliche, wie ja auch deren Abtrennung von den der oberkreidischen Cosmoceratiden-Gattung *Scaphites* eignenden *Striaptychi*, die sich strukturell und habituell ja kaum von den obigen *Praestriaptychi* unterscheiden lassen.

Wenn wir 1928, p. 135, als die wesentlichsten und staendigsten Merkmale der *Scaphites*-Opercula oder *Striaptychi s. str.* ihre duenne (kalkige) Schalenbeschaffenheit und die deutlich entwickelte konzentrische Runzelung oder Rippenstreifung ihrer Klappenkonvexseite hingestellt haben, so gilt dies wohl auch fuer ihre jurassischen Vorlaeufer, unsere *Praestriaptychi*. Auch sie sind zweivalvige zartschaligkalkige Aptychen mit konzentrischen Runzeln oder Streifen auf der Klappenkonvexseite. Gelegentlich koennen auch hier feinste Granulationen (Knoetchen) erscheinen. Eine kohlige (urspruenglich hornige) Schichte an der Konkavseite der Kalkschale ist nur ganz selten erhalten geblieben.

Praestriaptychi haben sich besonders im Dogger (vom Dogger δ an) und im Malm Sueddeutschlands und gelegentlich im Neokom des Teutoburger Waldes, der Schweizer und franzoesischen Alpen und auch Mexikos vorgefunden

GRANULAPTYCHUS Trauth (1927, pp. 217, 228, 233, 240 ; 1930, pp. 387–395 Taf. V Fig. 1–10 ; 1937, pp. 152–159 Taf. 11 fig. 5–16).

Ad *Stephanoceratidae* : *Perisphinctes* ?, *Stephanoceras* ?, *Holcostephanus* (*Astieria*)

ad *Cosmoceratidae* : *Garantiana* (*Subgarantiana*) ? *Hoplites*.

Der, wie schon vorhin bemerkt, neben dem ihm nahestehenden *Praestriaptychus* als Operculum den *Stephanoceratidae* und den ihnen verwandten *Cosmoceratidae* zukommende und namentlich bei der Ammonitengattung *Perisphinctes* erscheinende Opercular-Typus *Granulaptychus* hat sich vom Mitteldogger (δ/ε) an bis in den oberen Malm (ζ) Wuerttembergs und Bayerns sowie im Oberjura der Lombardei und auch einmal im Callovien Suedamerikas (Caracoles in Chile) vorgefunden.

Der Begriff der *Granulaptychi* laesst sich etwa folgendermassen fassen (1930, p. 388) : " Zweiklappige, kalkige, duennschalige Aptychen, in der Regel flach oder maessig gewoelbt, die Konvexseite mit mehr oder minder gleichmaessig verteilten oder aber in konzentrischen Reihen angeordneten feinen Knoetchen oder auch etwas groeberen Stachelwarzen bedeckt ; die erwaehnten Knotenreihen zuweilen auf flachen, ihren Sockel bildenden Runzeln stehend, wogegen sonstige konzentrische Runzelfalten oder Streifen (zum Unterschiede von *Praestriaptychus*) ganz zu fehlen pflegen. Die Valvenkonkavseite zeigt deutliche konzentrische Zuwachsrunzeln und-linien und zuweilen auch (namentlich bei den Malm-Formen) gegen den Externrand hin eine zarte Radialstreifung." Obzwar wir die Konkavseite der zarten Kalkschale nur bei einer einzigen Form, dem *Granulaptychus spinogranulosus* Trth., von einem duennen kohligen (wohl urspruenglich hornigen) Haeutchen ueberzogen gefunden haben, duerfte der Besitz eines solchen Haeutchens urspruenglich doch wohl allen oder immerhin einem Grossteil ihrer Formen zugekommen sein.

LAEVILAMELLAPTYCHUS Trauth (1930, pp. 363–377 Taf. III Fig. 1–3, Taf. IV Fig. 1–13 ; 1936*a*, pp. 36–39 Taf. III Fig. 11–17 ; 1936*c*, pp. 127–145 Taf. III).

Dieser mit *Harpoceras* (s.l.) im Oberlias und ferner mit *Hecticoceras*, *Oppelia*, *Haploceras*, und *Pseudolissoceras* im Dogger und Malm verknuepfte und schliesslich wohl auch noch im Neokom vorkommende Opercular-Typus ist wohl aus dem vorbesprochenen *Cornaptychus* bei Verlust der kohligen Unterschicht, resp. aus *Laevicornaptychus* oder aus *Lamellaptychus* hervorgegangen.

Er umfasst zweiklappige, zart-bis kraeftigschalige, kalkige Valven, welche bezueglich des Schalenbaues und der Beschaffenheit der Konkavseite im Wesentlichen den *Lamellaptychi* gleichen, aber durch das gaenzliche oder doch weitgehende Glattwerden der Konvexflaeche — sei es infolge primaerer Rueckbildung der konvexseitigen Berippung, sei es infolge der Ausbildung einer stellenweise (bis am Apex) rel. dicken, feinblaettrigen und glatten (sozusagen *punctaptychus*-oder *laevaptychus*-artigen Schalenoberschicht — von ihnen abweichen und von den *Punctaptychi* sich durch das Fehlen von " Interkostalroehren " unterhalb der Obserschicht und von " Punktloechlein "-Reihen in, resp. auf derselben unterscheiden.

Das Verbreitungsgebeit von *Laevilamellaptychus* umfasst nach den bisherigen Feststellungen den oberen Lias Wuerttembergs (Lias ζ) und der Kammerker in Nordtirol, den Dogger Wuerttembergs, der Schweiz, Frankreichs und Englands, den Malm (bez. Tithon) Sueddeutschlands, der Nordalpen (Klippenzone) und Suedalpen (Lombardei) und der argentinischen Anden und das Neokom der Cap Verden (Insel Mayo).

LAMELLAPTYCHUS Trauth (1927 pp. 216, 228, 233, 237–239 ; 1930, pp. 354–363 Taf. III Fig. 17–28 ; 1936*b*, pp. 66–76 ; 1938, pp. 115–229 Taf. IX–XIV).

Im Dogger noch ziemlich selten und auch bloss durch relativ wenige, wohl aus den *Cornaptychi* des Lias und untersten Doggers hervorgegangene " Formen " vertreten, werden die *Lamellaptychi* im Oberjura und der Unterkreide (Neokom) zu dem weitaus haeufigsten und formenreichsten Aptychentypus, der uns in diesen Formationsabteilungen bisher etwa ebensoviele " Formen " und " Varietaeten " geliefert hat wie alle anderen sonst noch darin auftretenden Aptychentypen zusammengenommen.

Die Definition von *Lamellaptychus*, der namentlich dem Ammonitengenus *Oppelia* weiteren Sinnes und daneben noch *Haploceras* p.p. (incl. *Lissoceras*) und vielleicht auch *Hecticoceras* ? zugehoert, mag nun folgendermassen lauten (1938, pp. 123–124) : " Zweiklappige, zumeist maessigstarke kalkige Aptychen, die aus einer sehr duennen und dabei recht kompakten (d.h. nicht zellig-poroesen) Unter- und Oberschichte und einer ganz wesentlich dickeren und die Hauptmasse der Schale darstellenden, fast stets zellig-tubuloes struierten Mittelschichte bestehen und deren Konvexseite " (die Oberschichte bildend) " ziemlich kraeftige, durch Furchen getrennte, mehr oder minder deutlich schraeg (" imbrikat ") gestellte, miteinander im allgemeinen parallel laufende Leistenrippen (" lamellose " Rippen) aufweist, waehrend die Konkavflaeche der zarten kalkigen " Unterschichte " nur ziemlich schwache konzentrische Anwachsrunzeln und-linien darbietet. Ein Adsymphysalsaum erscheint hier an der Konkavseite gewoehnlich deutlich entwickelt.

Lamellaptychi finden sich im alpinen wie im ausseralpinen Bereiche der Jura-und Neokomablagerungen Europas und der uebrigen Erdteile.

PUNCTAPTYCHUS Trauth (1927, pp. 217, 228, 233, 239–240 ; 1930, pp. 377–378 ; 1931, p. 22 ; 1935*b*, pp. 310–332, Textfig. 1–2 Taf. XII).

Dieser dem *Lamellaptychus* ueberaus nahestehende undo ffenbar aus ihm hervorgegangene Operculartypus, welcher sich eben ausser dem *Lamellaptychus* und *Laevilamellaptychus* bei dem Ammoniten-genus *Haploceras* (incl. *Pseudolissoceras*) vorfindet, vermutlich aber auch den freilich vorherrschend *Lamellaptychi* besitzenden *Oppeliae* gelegentlich — naemlich vereinzelten Arten derselben — zukommt, erscheint vielleicht schon im oberen Dogger (Bathonien oder Callovien), sicher aber erst im Oxfordien, um hierauf im Kimmeridge und Tithon seine groesste Haeufigkeit zu erlangen und schliesslich im Unterneokom (Berrias-Valendis) mit etwa zwei Formen abzuschliessen, wobei es von besonderem Interesse ist, seine Verbreitung wohl ausschliesslich auf die alpin-mediterrane Provinz (suedliches Europa und Nordafrika) beschraenkt zu sehen.

Der Typus *Punctaptychus* mag kurz in folgender Weise charakterisiert werden (1935*b*, p. 310) : " Zweiklappige, kalkige und verhaeltnismaessig kraeftigschalige Aptychen mit weitgehend den Lamellaptychen entsprechendem Valvenbaue und auch mit einer *lamellaptychus*-artigen Imbrikationsberippung, ueber die sich aber — ueber Rippen wie ueber deren Zwischenfurchen — zum Unterschiede von *Lamellaptychus* im zirkumapikalen Hauptareale der Valvenkonvexflaeche die bei letzterem Typus nicht oder kaum zur Entwicklung gelangende feinstblaettrig struierte " Decklage " (obere Partie) der Schalenoberschicht als ein ziemlich glatter und einheitlicher Ueberzug ausbreitet, nur im Dache der Rippenzwischenfurchen je eine Reihe " punktfoermiger " Loechlein (Punktloechlein, Punktationen) offen lassend.

LAEVAPTYCHUS Trauth (1927, pp. 217, 233, 242 ; 1930, pp. 395–403, 409, Taf. IV Fig. 14–19 ; 1931, pp. 23–136 Taf. I).

Dieser den *Aspidoceratidae* und zwar deren Gattungen *Aspidoceras*, *Waagenia* und vielleicht auch *Simoceras* zugehoerige Aptychen-Typus, der unseres Wissens zuerst im oberen Callovien und unteren Oxfordien des Juragebirges, Ostfrankreichs und Schwabens und dann, bei fortschreitender Dickschaligkeit auch ziemlich haeufig werdend, im ganzen uebrigen Malm (Oberoxford-Tithon) sowohl des ausseralpinen als des alpin-mediterranen Europas wie im Oberjura von Suedarabien, Ost-und Nordafrika (Somaliland, Tunis) und Amerika (Mexiko, argentin. Cordillere) angetroffen worden ist, laesst sich des Wesentlichen etwa folgendermassen definieren : " Zweiklappige, urspruenglich nur maessigstarke, spaeter dickschalig werdende kalkige Aptychen, aus einer schwachen dichten Unterschichte mit konzentrischen Zuwachsstreifen undrunzeln an ihrer Konkavseite, dann aus der rel. kraeftigsten — meist sogar sehr dicken — maschigzellig struierten, sehr widerstandsfaehigen Mittelschichte und endlich aus einer ueberaus zarten und hinfaelligen, daher oft nur mangelhalft oder gar nicht erhalten gebliebenen, scheinbar dichten und nur mit winzigen kleinen Poren besetzten, oberflaechlich glatten Oberschicht bestehend " (1930, p. 396).

Naehere Darlegungen ueber Schalenbau haben wir in unseren obzitierten Studien (bes. in 1931, pp. 24–31) geboten.

Aptychus sp. (vgl. Trauth, 1930, pp. 403–405).

Wie wohl im Palaeozoikum (p. 1) haben wir nun auch aus dem Mesozoikum und zwar aus der Juraformation das Auftreten einiger zweivalviger Ammoniten-Opercula zu erwaehnen, deren unzureichender Kenntnisstand leider nicht ihre Zuweisung zu bestimmten der eben in dieser Epoche unterschiedenen Typen erlaubt und die wir deshalb einfach bloss unter der ganz allgemeinen Bezeichnung "*Aptychus* " angefuehrt haben wie z.B. den *Aptychus* (?) *amistus Greg.* aus dem Aalenien des Monte Grappa in den Venezianer Alpen (vgl. 1930, p. 403).

Wir wenden uns nun den Aptychen der Oberkreide zu (1927, p. 228 ff. ; 1928, pp. 121–193 ; 1930, pp. 339–344).

RUGAPTYCHUS Trauth (1928, pp. 122–130, Taf. II Fig. 1–9).

Ein im Senon und zwar besonders in den obersenonen Mucronaten-Schichten von Sued-Schweden, Nord-Deutschland, Nord-Frankreich, Belgien, Sued-England und ev. Daenemark vorkommender und der

Ammonitengattung *Baculites* zugehoeriger, rel. starkschaliger *Aptychus* besitzt meist langgestreckte und leichtgewoelbte Valven, deren Konvexseite mit ziemlich kraeftigen Rippen bedeckt ist, welche haeufig durch eine nahe dem Externrand erfolgende hakenfoermige Biegung an die des neokomen *Lamellaptychus angulocostatus* (Pet.) erinnern, jedoch abweichend davon groesstenteils mit Knoten oder Koernchen besetzt und z.T. unregelmaessig wellig verbogen erscheinen.

LISSAPTYCHUS Trauth (1928, pp. 173–180, Taf. II Fig. 10–18)

Aus dem Cenoman und Turon Boehmens und aus dem Turon und Senon (Santonien-Campanien) des noerdlichen Europas (in England, Schweden, Daenemark, Norddeutschland und Podolien) bekanntgeworden und wenigstens hinsichtlich einer seiner Formen (*L. leptophyllus* [Shrp.]) als wahrscheinlichst der Ammonitengattung *Parapuzosia* (resp. der Art *P. leptophylla* Shrp.) zugehoerig erwiesen, besitzt dieser leider noch nicht voll erforschte Aptychen-Typus als bezeichnendste Merkmale eine ziemlich betraechtliche Zartheit der Schale und eine den Typus-Namen mitbedingende glatte Beschaffenheit von deren Konvexseite. Die ganz duenne konkavseitige Unter-und die fast ebenso zarte knovexseitige Oberschicht werden an Staerke merklich von der Mittelschicht uebertroffen, welche eine Lamellenstruktur aufweist, gebildet durch ihre zarten, zur Valvenoberflaeche parallel liegenden Aufbaublaetter.

PSEUDOSTRIAPTYCHUS Trauth (1927, p. 233 ; 1928, pp. 165–173, Taf. IV Fig. 1–12)

Da dieser der Ammonitengattung *Pachydiscus*, resp. *Parapachydiscus* entsprechende Operculartypus derzeit nur durch einen einzigen ganz sicheren Vertreter, den *Ps. pseudo-Stobaei* Trth., repraesentiert erscheint, waehrend die Zugehoerigkeit seiner drei weiteren dazu gerechneten Repraesentanten, des *Ps. Gollevillensis Trth.*, *Ps. Icenicus Trth.* und *Ps. Portlocki Trth.* wegen der nicht ganz sicher ausschliessbaren Moeglichkeit ihrer Zuweisung zu dem *Scaphites*-Aptychus *Striaptychus* noch etwas zweifelhaft bleibt (Trth., 1928, p. 166), kommt der von uns auf sie alle gegruendeten Charakterisierung des Typus *Pseudostriaptychus* naturgemaess ein bloss vorlaeufiger, provisorischer Inhalt zu, der durch kuenftige Funde noch sehr wohl eine merkliche Abaenderung erfahren koennte.

Unter der Voraussetzung also der Zugehoerigkeit saemtlicher vier obgenannter und aus dem Senon (den untersenonen *Quadratus*-Schichten [Santonien] und obersenonen *Mucronatus*-Schichten [Campanien]) von England und Norddeutschland stammender Aptychenformen als Opercula zu den *Pachydisei*, resp. den diesen naechstverwandten *Parapachydisei* laesst sich der Typus "*Pseudostriaptychus*" etwa in nachstehender Weise definieren : Schale sehr zart und daher fuer eine gute Konservierung wenig geeignet. Klappenwoelbung zwar deutlich, aber meist doch nur von maessiger Staerke. Konvexseite der

Valven in der Regel mit feinen konzentrischen Rippenstreifen (resp. feinen " Imbrikationsrippen ") bedeckt, mitunter aber (bei *Ps. Portlocki*) mit flachimbrikaten, breiten, konzentrischen Rippenbaendern ; Konkavseite mit sehr zarten konzentrischen Zuwachstreifen. Adsymphysalsaum gewoehnlich deutlich entwickelt. Valvenform langgestreckt (bei *Ps. Portlocki* L : B= 3·2 : 1, bei *Ps. Gollevillensis* L : B=2·5 : 1) oder von maessiger Laenge (bei *Ps. Portlocki* L : B=1·7 : 1 bis 1·5 : 1) oder ziemlich gedrungen (bei *Ps. pseudo-Stobaei* L : B=1·25 : 1). Feinstruktur der Schale nicht bekannt und wohl — wenn vorhanden gewesen — infolge calcitischer Umkristallisierung verloren gegangen.

Die morphologische Aehnlichkeit mit den *Striaptychi* von *Scaphites* ist demnach wohl eine sehr weitgehende (vgl. im Folgenden).

STRIAPTYCHUS Trauth (1927, pp. 219, 229 ; 1928, pp. 134–165, Taf. III Fig. 1–16, Taf. IV Fig. 13–27).

Dieser als Operculum der Ammonitengattung *Scaphites* zukommende und aus den Turon und Senon von Daenemark, Norddeutschland Sachsen und Boehmen sowie aus dem Senon (besonders Obersenon) des ausserkarpathischen Galiziens, Englands und Nordamerikas sowie in vereinzelten Exemplaren schliesslich aus dem Oberkreideflysch (wohl besonders Senon) der bayrischen und niederoesterreichischen Flyschalpen bekanntgewordene Aptychus gleicht wohl weitgehend dem den Ammonitengattungen *Pachydiscus*, resp. *Parapachydiscus* eignenden und vorhin gekennzeichneten Typus *Pseudostriaptychus* und besitzt so eine duenne oder doch eine ziemlich duenne Schalenbeschaffenheit mit einer deutlich entwickelten konzentrischen Runzelung oder Streifung der Klappenkonvexseite. Die Valven-Konkavseite ist gewoehnlich mit concentrischen Anwachsrunzeln oder -streifen und zuweilen (so bei *Striaptychus cretaceus* [Muenst.] und *Str. radiatus* [Fr.]) auch mit einfachen oder sich veraestelnden Radialstreifchen versehen. Bei ihrer naeheren, speziellen Beschreibung haben wir die verschiedenen *Striaptychus*-Formen einer besseren Uebersichtlichkeit wegen in vier auf die Skulpturentwicklung gegruendete Gruppen zerlegt, naemlich in a) Formen mit konzentrischer Skulptur, aber ohne Radialstreifung und ohne Granulationen auf der Schalen-Konvexseite, b) Formen mit konzentrischer Skulptur und mit Radialstreifung, aber ohne Granulationen auf der Schalen-Konvexseite, c) Formen mit konzentrischer Skulptur und mit Granulationen, aber ohne Radialstreifung auf der Schalen-Konvexseite und d) Formen mit konzentrischer Skulptur, Radialstreifung und Granulationen auf der Schalenkonvexseite.

Die weitgehende Uebereinstimmung des Schalenbaues von *Striaptychus* mit dem des jurassisch-neokomen *Praestriaptychus* laesst den ersteren unschwer als Nachfahren des letzteren erkennen.

SPINAPTYCHUS Trauth (1927, pp. 220, 232 ; 1928, pp. 130–134, Taf. III, Fig. 17–18 ; 1930, pp. 339–344, Taf. V, Fig. 19–20)

Dieser im Senon (etwa oberem Coniacien bis unterem Campanien) von England, Palaestina und Syrien (Libanon) angetroffene und wohl von Angehoerigen der Ammonitenfamilie *Prionotropidae* Zitt. (etwa von deren Gattungen *Mortoniceras* ? und *Schloenbachia* ?) stammende Operculartypus ist an seiner Konvexseite namentlich durch den Besitz von teils ungleichmaessig, teils aber auch reihig gruppiert auftretenden Hohlwarzen (hohlen Stachelwarzen) ausgezeichnet.

Die Valvenkonkavseite zeigt namentlich feine konzentrische Anwachsrunzeln, die mitunter auch von etwas groeberen und ihnen parallel laufenden Runzeln begleitet werden.

Von den mittel-und oberjurassischen *Granulaptychi* unterscheidet sich *Spinaptychus* namentlich durch die hohle scheitelgeoeffnete Beschaffenheit der Oberflaechenwarzen.

CRASSAPTYCHUS Trauth (1927, pp. 221, 232 ; 1928, pp. 180–182, Taf. III Fig. 19–20).

Als einziger Vertreter dieses hinsichtlich seiner Zugehoerigkeit zu einem bestimmten Ammoniten-Genus noch voellig unbekannten Aptychen-Typus ist der aus der weissen Kreide (den senonen Mucronaten-Schichten) von Meudon bei Paris bekanntgewordene *Aptychus crassus* Héb. zu betrachten, dessen leider noch unzureichende, eben ausschliesslich auf Hébert's Darstellung (1885) beruhende Kenntnis vorlaeufig die Charakterisierung des Typus einigermassen problematisch macht : Die wesentlichen, eine Unterscheidung von allen uebrigen Kreideaptychen leicht ermoeglichenden Eigenschaften des *Crassaptychus* sind die rel. ungewoehnliche, anscheinend vom Lateral-gegen den Symphysalrand (eine Strecke weit) noch ein wenig zunehmende Schalendicke, resp. die Staerke der " mittleren " Schalenschicht und deren tubuloese, etwa an die des oberjurassischen *Laevaptychus*-Typus erinnernde Struktur. Die Innenschicht erscheint duenn und abgesehen von ganz schwachen parallelen, bezueglich konzentrischen Streifen glatt (vgl. E. Hébert : Tableau des fossiles de la Craie de Meudon et description de quelques espèces nouvelles. Mém. de la Soc. géol. de France V. tome, p. 367 [Paris 1855]).

Aptychus sp.

Einige oberkreidische Ammonitendeckel von mangelhaftem, zu einer sicheren Zuweisung zu einem der vorhin unterschiedenen Operculartypen nicht oder kaum ausreichenden Kenntnisstand haben wir 1928, pp. 182–183 angefuehrt und zwar aus dem ebenerwaehnten Grunde einfach unter dem allgemeinen Gattungs-(Typus) Namen *Aptychus*, z.B. *Aptychus Gravesianus d'Orb.* aus der weissen Kreide des Pariser Beckens. (Dies analog wie bei den ebenso mangelhaft ueberlieferten "*Aptychi* " aus den frueheren geologischen Zeiten, vgl. p. 1 und bes. p. 9.)

Veroeffentlichungen von F. Trauth ueber Aptychen	*Zitierl-mit*
Aptychenstudien I. Ueber die Aptychen im Allgemeinen. Annalen d. Naturhistor. Museums in Wien, Bd. 41, pp. 171–259 (m. 8 Textfig. u. 1 Tabelle, Wien 1927)	1927
Aptychenstudien II. Die Aptychen der Oberkreide. Ann. d. Naturhistor. Mus. in Wien, Bd. 42, pp. 121–193 Taf. II–IV (Wien 1928)	1928
Aptychenstudien III–V. Ann. d. Naturhistor. Mus. in Wien, Bd. 44, pp. 329–411 (Wien 1930). III. Nachtrag zu den "Aptychen im Allgemeinen" pp. 330–338 ; IV. Nachtrag zu den "Aptychen der Oberkreide" pp. 339–345, Taf. V, Fig. 19–20 ; V. Die Aptychen des Dogger, pp. 345–411, Taf. III, IV und V. Fig. 1–18	1930
Aptychenstudien VI–VII. Ann. d. Naturhistor. Mus. in Wien, Bd. 45, pp. 17–136 (Wien 1931). VI. Zweiter Nachtrag zu den "Aptychen im Allgemeinen" pp. 18–21 ; VII. Die Aptychen des Malm und der Unterkreide, pp. 22–23 ; Laevaptychus pp. 23–136 (Fig. B, p. 129, Fig. C, p. 131) Taf. I	1931
Die Aptychen des Palaeozoikums. Jahrb. d. Preuss. Geolog. Landesanst. f. 1934, Bd. 55, pp. 44–83, Taf. 2 u. 3 (Berlin 1934)	1934*a*
Die Anaptychen des Lias. N. Jahrb. f. Mineralog. etc., Beil-Bd. 73, Abt. B, pp. 70–99 (Stuttgart 1934)	1934*b*
Die Aptychen der Trias. Sitzungsber. d. Akad. d. Wiss. in Wien, mathem-naturw. Klasse, Abt. I, 1944, Bd. pp. 455–483 Taf. I (Wien 1935)	1935*a*
Die Punctaptychi des Oberjura und der Unterkreide. Jahrb. d. Geolog. Bundesanst, 85 Bd., pp. 309–332, Taf. XII (Wien 1935)	1935*b*
Anaptychi und anaptychus-aehnliche Aptychi der Kreide. N. Jahrb. f. Mineralog. etc., Beil. Bd. 74, Abt. B, pp. 448–468 (Stuttgart 1935)	1935*c*
Die zweivalvigen Aptychen des Lias. I. Teil. Jahreshefte d. Vereins f. vaterlaend. Naturkunde in Wuerttemberg, 91 Jahrg. 1935, pp. 22–58, Taf. VI (Stuttgart 1935)	1935*d*
Die zweivalvigen Aptychen des Lias. II. Teil. Jahreshefte d. Vereins f. vaterlaend, Naturkunde in Wuerttemberg, 92 Jahrg. 1936, pp. 10–44, Taf. III (Stuttgart 1936)	1936*a*
Ueber Aptychenfunde auf Cuba. Koninkl. Akad. van Wetensch. te Amsterdam, Proceedings Vol. XXXIX, No. 1, pp. 66–76 (A'dam 1936)	1936*b*

	Zitiert-mit
Aptychenstudien VIII. Die Laevilamellaptychi des Oberjura und der Unterkreide. Ann. d. Naturhist. Mus. in Wien, Bd. 47, pp. 127–145, Taf. III (Wien 1936)	1936*c*
Die Praestriaptychi und Granulaptychi des Oberjura und der Unterkreide. Palaeontolog. Zeitschr. Bd. 19, pp. 134–162, Taf. 10 u. 11 (Berlin 1937)	1937
Die Lamellaptychi des Oberjura und der Unterkreide. Palaeontograph, Bd. LXXXVIII, Abt. A, pp. 115–229, Taf. IX–XIV (Stuttgart 1938)	1938

DOCUMENT 1/42

A Supplementary Proposal arising in connection with the "Parataxa Plan"

(Commission Reference : Z.N.(S.) 1056)

Proposed insertion in the "Règles" of provisions recognising the giving of names to Collective Groups

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 16th October 1957)

When parasitologists transfer a species from a collective group to a genus, they appear to be accustomed to retain the name of the original author, contained within parentheses, and followed by the name of the author who made the transfer. This may be seen from a list of such transfers contained in a letter dated 13th August 1956 from Mr. Allen McIntosh published in the *Bulletin of Zoological Nomenclature* (15 : 94) on 11th September 1957[1]. The wording of Article 23 of the *Règles* does not authorize this, but should be amended to do so. To this end I propose that Article 23 be amended at the London Colloquium to read :

> When a species is transferred to another than the original genus or the specific name is combined with any other generic name than that with which it was originally published, or a specific name originally established in a collective group is transferred to a genus of different name, the name of the author of the specific name shall be retained in the notation but placed in parentheses.

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[1] The paper here referred to forms Annexe 7 to Document 1/14 on the London Agenda Paper.

2. This change could be fitted into the revised draft of the *Règles* by adding a new subparagraph (v) in Article 22, Section 5(c)[2], to read :

> The provisions of subparagraph (i) and (iv) shall apply equally in the case of a species established in a collective group when it is subsequently transferred to a genus of different name.

3. Since it is the custom among parasitologists to regard names of collective groups as entering into homonymy with generic names (cf. paragraph (2) in the letter from Allen McIntosh dated 9th December 1956 (*Bull. zool. Nomencl.* **15** : 97) a statement to that effect should appear in the *Règles*. For this purpose I propose that Article 34 be amended to read :

> A generic name, subgeneric name, or name of a collective group shall be rejected as a homonym when it has previously been used for some other genus, subgenus, or collective group of animals.

4. This could be fitted into the revised draft of the *Règles* by making the following modifications :—

Article 24, Section 7[3], to read :

> The Law of Homonymy shall apply equally to generic names, names of collective groups, and subgeneric names. It shall not apply between such names and those of the Order and Class Group or higher, nor between such names and those of taxa below subgenus.

Article 24, Section 8(a)[4], the first sentence to read :

> Generic names, names of collective groups, or subgeneric names of identical spelling are homonyms.

Article 24, Section 10(b)[5] of the proposed redraft of the *Règles* should be changed to commence :

> Homonymy among specific or subspecific names shall apply only to those that are or have been contained in the same nominal genus or collective group, thus . . .

5. The first phrase of Article 6, Section 1(h)[6] of my proposed draft of the *Règles* reads : A specific, subspecific, or infra-subspecific name must be published

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[2] See Vol. **14** : 163.

[3] See Vol. **14** : 175.

[4] See Vol. **14** : 176.

[5] See Vol. **14** : 106.

[6] See Vol. **14** : 49.

in connection with a specified generic name. This is a statement, in different wording, of the rule of availability that an author must have applied the principles of binominal nomenclature (*Règles*, Article 25(b), as amended at Paris, *Bull. zool. Nomencl.* **4** : 65, paragraph (3)(a)(i)). In order to make provision for collective groups, this should be reworded as follows :

> A specific, subspecific, or infra-subspecific name must be published in connection with a specified generic name or a name of a collective group.

6. It would seem desirable to add the following paragraph to Article 1, Section 3[7] of the proposed revised draft of the *Règles* (cf. *Bull. zool. Nomencl.* **15** : 97(3)).

> (a) *Collective groups.* A collective group, although not a natural taxonomic group, shall not be considered to be a hypothetical concept.

7. The following paragraph should be added to Article 25, Section 3 of Proviso (c) of the *Règles*, as amended at Paris (*Bull. zool. Nomencl.* **4** : 72, paragraph (8)(1)) and equally to Article 6, Section 1(k)[8] of the proposed draft of the *Règles* :

> The provisions of paragraph (k) do not apply to names of collective groups.

8. The following paragraph should be added to the recommendation in Article 8 of the *Règles*, made a mandatory provision at Paris (cf. *Bull. zool. Nomencl.* **4** : 253, paragraph 15) and equally at the end of Article 14, Section 2[9] of the proposed draft of the *Règles* :

> Wherever the word " taxon " is employed at the generic level in the *Règles*, it should be understood that the provision in question applies equally to a collective group, except where it would be inappropriate, or is distinctly excluded by the purport of the provision.

9. I believe that the foregoing proposals provide for the substance of the resolution of the American Society of Parasitologists (adopted at a business meeting of the Society, 29th August 1956, by a unanimous vote estimated to have been over 200 persons) (cf. *Bull. zool. Nomencl.* **15** : 90) as well as for the points discussed in the letter from Mr. Allen McIntosh dated 19th December 1956, to which reference has already been made (*Bull. zool. Nomencl.* **15** : 96–98), and such additional matters as are necessary for the sanction and operation of the relevant usages of parasitologists.

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[7] See Vol. **14** : 36.

[8] See Vol. **14** : 51.

[9] See Vol. **14** : 101.

DOCUMENT 1/43

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **JOHN S. HAMPTON**

(*Bromley, Kent*)

(Letter dated 12th November 1957)

The recent proposals by Professor R. C. Moore and Mr. P. C. Sylvester-Bradley, "Proposed Recognition of the Concept 'Parataxon' and the Provision of Rules for the Nomenclature of Units of this Category" (1957, *Bull. zool. Nomencl.* **15** : 5–13) appear to offer complete clarification of nomenclatural and taxonomic problems which have arisen in micropalaeontological studies of certain fragmentary fossils. In support of the above workers proposed recognition of " Parataxa " in the *Règles*, I furnish the following comments concerning the discrete fossil sclerites of Holothuroidea (Echinodermata) :

(1) Dissociated sclerites are the only basis for an understanding of the palaeontology of the class Holothuroidea. They are widely distributed in marine sedimentary strata, but are seldom common (Frizzell and Exline, 1955, *Bull. Missouri School Mines and Metallurgy*, No. 89). The study of fossil sclerites presents, however, an almost completely unexplored field in micropalaeontology, as no suitable procedure has been provided for applying names to them without reference to the whole-animal species which they represent. The dual nomenclature now applied (see Frizzell and Exline, *op. cit.*) has to some extent solved this problem, but the recognition of the " families, genera and species " of Frizzell and Exline's (*op. cit.*) classification of fossil sclerites as parataxon would make the study of such discrete fragments of considerable value in stratigraphical palaeontology.

(2) Many of the " genera " of fossil holothurian sclerites are extinct and no comparisons can be made to Recent biospecies. This would, however, be impossible as in Recent holothurians markedly unlike sclerites are found within the same species, and apparently identical sclerites are reported to exist in

relatively unrelated forms. It appears obvious, therefore, that fossil holothurian sclerites are best fitted only for parataxa. Frizzell and Exline recognise the categories of their classification (*op. cit.*) as parataxa (Frizzell, 1957, *Personal Communication*).

(3) In recent micropalaeontological studies several new "genera, subgenera and species" of holothurian sclerites have been proposed (Hampton, *Geol. Mag.*, and *Micropaleont.*, in press), the artificial nature of these categories was recognised, and as they were proposed within the classification of Frizzell and Exline (*op. cit.*) they are best considered as parataxa.

(4) The taxonomic arrangement adopted by Frizzell and Exline (*op. cit.*) is exactly comparable to that of Zoology (i.e. Linnean taxonomic categories are employed), and to avoid ambiguity the recognition of fossil categories as parataxa is essential.

As a worker on the micropalaeontology of holothurian sclerites I strongly support the proposals of Moore and Sylvester-Bradley (*op. cit.*) to apply to the study of certain discrete fragments.

CASE No. 2

(*continued from page* 154)

DOCUMENT 2/16

Submission of a Draft Text embodying the reversal of Copenhagen Decision 54(1)(a) and of the amalgamation of the provisions so amended with Decision 54(1)(b)

By **G. H. E. HOPKINS**

(*British Museum* (*Natural History*), *The Zoological Museum, Tring*)

(Commission Reference : Z.N.(S.) 835)

(Proposal submitted under cover of a letter dated 14th August 1957)

Until relatively recently the International Rules of Zoological Nomenclature made no provision for the application of priority to names other than those of genera and smaller taxa, and until 1953 there was no clear provision for the mode of application of priority to family-group names. In these circumstances authors have applied priority to such names as best they could in the absence of generally-accepted rules.

2. The Rules decided on at Copenhagen in 1953 (*Copenhagen Decisions on Zoological Nomenclature* : 32–37, Decisions 43–58) have cleared up many obscurities, but one point is still not sufficiently clear and one decision seems to me to introduce an unnecessary complication into the Rules and to represent an unfortunate departure from the principle which had been followed for very many years, while another point is still not sufficiently clear.

3. Both the points I wish to discuss are contained in Decision 54 of the Copenhagen *Decisions* and deal with family-group names found to be based on generic names which are invalid, either because they are junior synonyms (objective or subjective) (Decision 54(1)(a)) or because they are junior homonyms (Decision 54(1)(b)). In the latter case the family-group name based on a junior homonym is to be altered, whereas in the former instance, provided for in Decision 54(1)(a), the family-group name based on a junior synonym is not to be changed. This difference in the treatment to be accorded to family-group names based on generic names which are invalid for different reasons seems to me to be illogical and to introduce an unnecessary complication. Moreover, the provision that family-group names based on generic names which are synonyms should not be altered is in conflict with a principle contained in the earliest proposals for an international Code of Zoological Nomenclature which include any provision on the point (1897, *Bull. Soc. zool. France.* **22** : 179, " Un nom de famille doit disparaitre et être remplacé, si le nom génerique, aux dépens duquel il était formé, tombe en synonymie et disparait luimême de la nomenclature "), which was contained in the accepted version of the *Règles Internationales de la Nomenclature Zoologique* published in Paris in 1905 (Article 5, p. 29, " The name of a family or subfamily is to be changed when the name of its type genus is changed "), and which was in force until 1953. That the reversal of a principle which has been followed by two generations of zoologists should apply retrospectively seems to me to be deplorable, and in the group of insects with which I am mainly concerned will, if not rectified, force us to choose between making a number of totally unnecessary changes in family-group names which are universally accepted and which were valid under the Rules in force when they were proposed, or making a number of appeals to the Commission that these names should be validated. From Dr. W. J. Arkell's remarks (1957, *Opin. Decl. int. Comm. zool. Nomencl.* 16 : (v))[1] it is clear that the same position exists in many other groups of animals besides that which is my main concern.

4. The obscurity in the Copenhagen Decisions regarding family-group names to which I have referred is in Decision 54(1)(b). On this subject I am submitting a separate note.[2]

5. My proposal as regards the present part of the subject is that Section (1) of Decision 54 of the *Copenhagen Decisions on Zoological Nomenclature* should be rescinded and replaced by the following :—

> The name of a taxon belonging to the family-group must be based on the oldest available name for the type genus ; and accordingly where the name of such a genus is rejected either (i) as a junior synonym (either objective or subjective) of an older generic name or (ii) as a junior homonym of an older generic name, the name of the family-group taxon based on the rejected generic name is itself to be rejected.

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[1] See also Document 2/1.

[2] See Document 3/2.

DOCUMENT 2/17

Submission of a Draft Text embodying a partial reversal of Copenhagen Decision 54(1)(a)

(Commission Reference : Z.N.(S.) 835)

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Statement received on 15th October)

When should a rejected junior synonym continue to serve as the basis for a name of the Family-Group ?

The Copenhagen Decisions on Zoological Nomenclature, Decision 54(1)(a) (: 36) provide that a family name is not to be changed because the name of its type genus is found to be a junior synonym.

2. This action has led to widespread dissatisfaction. Numerous applications received by the Commission since 1953 strongly condemn it, and I have not observed a single case where its wisdom has been upheld. In practise it has been found that if followed, it would lead to name-changing, because in the past it has been quite the universal custom to assume that the name of a taxon of the Family-Group must be based on the valid name of its type-genus, and changes based on that assumption have become firmly fixed in literature.

3. The action was taken to prevent name-changing, but certainly the minds of its sponsors were looking to the future, and did not at all envisage the overhauling of current names that it would entail. It was correctly foreseen that the well-established name of many a Family-Group taxon would, from time to time be found to be based on an objective or subjective junior synonym of another generic name. It was designed to prevent the necessity of thereupon making a corresponding change in the name at the Family-Group level. In this respect the Copenhagen action is not without merit. It may readily be seen that it is particularly important in case the synonymy is subjective and not universally accepted, for different taxonomists might otherwise feel obliged to use different names at the Family-Group level for the taxon in question.

Where the name of the type-genus of a nominal Family-Group Taxon is a Junior Synonym Z.N.(S.) 835

4. At the outset it will be worth while to consider the various ways in which a Family-Group name could logically be handled within the general principles of zoological nomenclature if the name of its type-genus is a junior synonym, first for objective, then for subjective synonymy.

(a) If the nominal species which is the type-genus of a family is a junior objective synonym

5. Suppose that the nominal genus *A-us*, 1798, with its type species *x*, is the type-genus of the nominal family A-IDAE, 1815. In 1900 *B-us*, 1796, is found also to have the type species *x* and therefore to be an objective synonym of *A-us*. In such a case, rules aside, there are four logical courses of action :

(1) If a new nominal family B-IDAE were to be founded in 1900, this would have the type-genus *B-us*, a different nominal genus from *A-us*, and therefore would date from 1900. B-IDAE would have to compete in priority with all other Family-Group names within the same taxonomic family. Thus if the first such taxon to be established had been a subfamily C-INAE in 1875, C-IDAE would now become the family name to replace A-IDAE. A-IDAE would become an unavailable* senior objective synonym of B-IDAE, and at the family level, an unavailable senior subjective synonym of C-IDAE. B-IDAE would become an available junior subjective synonym of C-IDAE. An advantage of this procedure is that the family name would not be a wholly unknown Family-Group name.

(2) A shift from the name A-IDAE, 1815, to B-IDAE, 1900 would involve a change in the nominal type-genus of the taxonomic family, but no taxonomic difference could possible arise. It would therefore be reasonable for the *Règles* to provide that since the family B-IDAE is objectively the taxonomic equivalent of A-IDAE, it shall rank from 1815 and supplant A-IDAE. This would have the disadvantage of almost certainly introducing a wholly unfamiliar name to replace a well-established family name.

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* On the supposition that the name of a taxon of Family-Group is not an available name unless the name of its nominal type-genus is valid.

(3) *A-us* is the nominal type-genus of A-IDAE, 1815, and there is no provision of the *Règles* to the effect that a nominal family must be abandoned in case its nominal type-genus is incorrectly named. Therefore it is quite logical to rule that the nominal family A-IDAE in such a case shall be maintained, even though its nominal type-genus *A-us* exists only as a junior objective synonym of *B-us*. This is the prevailing rule, and was adopted at Copenhagen in 1953 (cf. *Copenhagen Decisions zool. Nomencl.* : 36, Decision 54(1)(a)(i)). This plan has the advantage that it avoids any shift in the first-established family name for the taxonomic family. There is no objection to applying it to cases that may arise in the future, but the attempt to apply it to family names that have already been changed on the basis of some other understanding is unfortunate.

(4) The Commission may be requested to suppress *B-us* under its Plenary Powers, thus permitting the retention of *A-us* and A-IDAE.

(b) If the nominal species which is the type-genus of a family is a junior subjective synonym

6. Suppose that the nominal genus *A-us*, 1798, is the type-genus of the nominal family A-IDAE, 1815, with its type-species *x*. In 1900 *B-us*, 1796, type species *y*, is believed to be a subjective synonym of *A-us*. In such a case there are three logical courses of action :

(1) If a new family B-IDAE were to be founded in 1900, this would have the type-genus *B-us*, a *different* nominal genus from *A-us*, and only potentially synonymous with the taxonomic genus *A-us*. B-IDAE would have to compete in priority with all other Family-Group names within the same taxonomic family. Thus if the first such taxon to be established had been a subfamily C-INAE in 1875, C-IDAE would now become the family name to replace A-IDAE. A-IDAE would remain an available senior subjective synonym of C-IDAE, subject to revival only by anyone who treats *A-us* and *B-us* each as a valid taxonomic genus*. At the family level it would also be a senior subjective synonym of B-IDAE. As in Section (a) (paragraph 5(1) above) this method has the advantage of providing at the family level a not wholly unfamiliar Family-Group name ; but it has the disadvantage of providing only an unstable name subject to fluctuation with taxonomic opinion as to the synonymy between the genera *A-us* and *B-us*.

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* Although A-IDAE would be a senior subjective synonym of both B-IDAE and A-IDAE it could not be employed by anyone who accepts the synonymy of *A-us* and *B-us*. This follows from the assumption that the name of a taxon of the Family-Group is not an available name unless the name of its nominal type-genus is valid.

(2) The solution described under Section (a) in paragraph 5(2) above, will not apply in the case of subjective synonymy between *A-us* and *B-us*.

(3) *A-us* is the nominal type-genus of A-IDAE, 1815, and there is no provision of the *Règles* to the effect that a nominal family must be abandoned in case its nominal type-genus exists only as a subjective synonym of another generic name. Therefore it is quite logical to rule that the nominal family A-IDAE shall be maintained. This plan not only maintains the first-established family name, being therefore in compliance with the provision that priority shall obtain, but guards against the necessity of changing from one family-name to another whenever there is change of thought in regard to the synonymy between *A-us* and *B-us*. It is therefore even more important to use it in the future than it is when the synonymy is objective, but its application to cases which have been decided on some other basis in the past, as now required by the *Règles*, is causing trouble.

(4) As in the case of objective synonymy, the Commission could be appealed to, but would scarcely be inclined to suppress the older genus *B-us* ; since taxonomists who did not agree to the synonymy would wish to use it at the generic level. If they did not suppress *B-us*, they would either have to adopt A-IDAE for all taxonomists ; which would be the equivalent to plan (3) preceding, or adopt A-IDAE only for those who do not accept the synonymy of *A-us* and *B-us*, C-IDAE for those who do, which would be the plan under (1) preceding.

7. Although, for simplicity, I have described the preceding alternatives in terms of family, they apply equally to all categories of taxa of the Family-Group.

8. It is evident that, whatever plan may be adopted, it should be clearly stated to be the normal plan, especially applicable to cases that arise in the future. If a family name has already been changed because its nominal type-genus is an objective synonym, or held to be a subjective synonym, and the change has won any measure of acceptance, it should not be again disturbed. Other cases, or doubtful cases, should be presented to the Commission for decision, evoking the Plenary Powers if necessary.

9. When the nominal species which is the type-species of a genus is discovered to be a junior synonym, we do not change the name of the genus, nor do we change the type-species. The principle of the permanency of types forbids the latter. Any type, all of which serve to determine the proper application of the name of some taxon, would be useless if it were subject to change. What we *do* do is to list the nominal species which is the type-species as a junior synonym of some other nominal species, and if it be an objective synonym, we cite it in the *Official List of Generic Names in Zoology* under the name of its senior synonym. Here we find ample precedent for the principle that would be applied to Family-Group names by paragraphs (3) preceding. Furthermore only that method would observe the principle of priority in names of the Family-Group. It is therefore the only plan (outside of appeal to the Commission) that would consistently employ the oldest name, and therefore would cause the least change, unless applied to names that have already been changed by some other method.

10. From all these considerations, I am led to propose the following amendments to the *Règles*, for consideration at the London Congress :—

(1) To extend Decision 61(2) of the *Copenhagen Decisions on Zoological Nomenclature* to apply to all taxa of the Family-Group or higher, and to re-word it to read :

(a) The type of each taxon of the Family-Group or higher shall be a nominal genus.

Explanation : A taxonomic genus cannot serve as a type, because it is incapable of objective definition, except in terms of a nominal genus, and is therefore subject to varied interpretation and fluctuating limits.

(b) The type of any taxon once fixed, shall not be subject to change, except under the Plenary Powers of the Commission.

(c) A nominal type-genus is not necessarily a valid genus, but may have come to be listed as a junior synonym of another nominal genus.

Explanation : If the name of a taxonomic family is changed, as from A-IDAE to B-IDAE, the type of the nominal family A-IDAE has not been changed, but a new nominal family B-IDAE has been established, dating from the time of the change. B-IDAE is then a junior subjective synonym of A-IDAE as long as their type-genera are held to belong to the same taxonomic family.

[This would replace Article 18, Section 2[1], of the proposed redraft of the *Règles*.]

(2) Decision 54(1)(a) of the *Copenhagen Decisions* to be amended to read :

The name of the nominal type-genus of a nominal taxon of the Family-Group is found to be a junior synonym (whether objective or subjective), and a corresponding change has not customarily been made in the name of the taxon of the Family-Group, no change shall hereafter be made on that account in the name of the taxon of the Family-Group. But if such a change has already been made on grounds of such synonymy, and has won general acceptance, it shall be adopted as the correct name of the taxonomic Family-Group, and shall be given the same date as the supplanted name, of which it shall be deemed to be a senior synonym.

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[1] See Vol. **14** : 117

Doubtful cases, especially in regard to degree of acceptance, shall be submitted to the Commission for decision.

Explanation : If both names were not given the same date, the supplanting name would be a junior synonym, hence invalid under the Law of Priority.

[This would replace Article 13, Section 4(a),[2] of the proposed redraft of the *Règles*.]

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[2] See Vol. **14** : 97.

CASE No. 3

(*continued from page* 155)

DOCUMENT 3/2

Submission of a Draft Text providing for the reversal of the provisions in Copenhagen Decision 54(1)(b) in relation to the date and authorship to be attributed to a substitute family-group name

(Commission Reference : Z.N.(S.) 1252)

By **G. H. E. HOPKINS**

(*British Museum* (*Natural History*), *The Zoological Museum,*
Tring, *Herts*, *England*)

(Enclosure to a letter dated 14th August 1957)

The authorship and priority to be attributed to Family-Group names replacing others under Copenhagen Decision 54(1)(b) is not made sufficiently clear. Traub & Hopkins (1955, *Trans. R. ent. Soc. Lond.* **107** : 252) considered that " it is clearly implied that the replacing name inherits the seniority of that which it replaces ", and the same interpretation of the provision has been made by at least two other siphonapterologists, but the Secretary to the Commission on Zoological Nomenclature tells me that although this principle was submitted to the Copenhagen Congress (see Hemming, 1952, *Bull. zool. Nomencl.* 7 : 73, 74) it was rejected, so that our interpretation of Decision 54(1)(b) is incorrect.

2. It is obviously desirable that no change of family-group name necessitated by the discovery that the generic name on which a family-group name is based is invalid should be allowed to affect the identity of the taxon by bringing about a change in its type-genus, yet this will often be the case if Decision 54(1)(b) is to remain unaltered. Taking an example from the Siphonaptera, the family-group name SARCOPSYLLIDAE was published by Taschenberg in 1880 for a taxon based on the genus *Sarcopsylla* Westwood, 1840 (a junior objective synonym of *Tunga* Jarocki, 1838) ; the name TUNGIDAE was first published by C. Fox in 1925 but there is a much senior family-group name, HECTOPSYLLIDAE Baker, 1904, which most authors regard as a subfamily of TUNGIDAE ; in this instance Decision 54(1)(b) involves the setting up as the type-genus of the family of a genus belonging to a different subfamily from that to which the genus which has always been regarded as the type belongs.

In another instance (used below as an example) the family-name which has to be replaced is based on a generic name which is a junior homonym, but again the result of Decision 54 would be similar, for it would necessitate rejecting the universally accepted name of the family in favour of one based on a genus which certainly belongs to a different subfamily and which is regarded by some authors as belonging to a different family.

3. All siphonapterologists, in common with what I believe to be the great majority of zoologists, have taken the view that such changes in the basic concept of a family-group taxon are undesirable in the highest degree, and I can only assume that the rejection by the Copenhagen Congress of the principle that the identity of a family-group taxon must be maintained through all nomenclatorial vicissitudes must have been due to the obvious awkwardness of attributing to a name an authorship and date which are not in accordance with fact and which reference to the work cited would show to be incorrect. There is a very real difficulty and I have tried to deal with it in a further proposal which I am submitting herewith.[1]

4. In order to ensure that the type-genus of a family-group taxon shall not be changed because of some nomenclatorial discovery, I suggest the following provision :—

Where a family-group name is rejected on the ground that the name of its type genus is either (i) a junior synonym (either objective or subjective) of an older generic name or (ii) a junior homonym of an older generic name, the family-group name published in substitution for the name so rejected shall rank for the purposes of priority from the date on which the rejected family-group name for which it is a substitute was originally published.

Example : The family-group name CTENOPSYLLIDAE was published by Baker in 1905 for a taxon based on the generic name *Ctenopsyllus* Kolenati, 1863 (a junior homonym of *Ctenopsyllus* Kolenati, 1857), for which *Leptopsylla* Jordan & Rothschild, 1911, is a *nomen novum* and the oldest available name ; the name LEPTOPSYLLIDAE was first published by Rothschild in 1915. The priority of this family would date from 1905.

PURCHASED 17 JAN 1958

Corrigenda

The following corrections should be made on page 154 of the present volume :—

line 3 : substitute "ALSATASPIDAE" for "ALASTASPIDAE"

line 4 : substitute "is" for "as"

line 6 : substitute "ALSATASPIDAE" for "ALSATASIDAE"

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[1] See Document 4/1.

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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* Professor Esaki died on 14th December, 1957, while the present Part was passing through the press.

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CASE No. 11

DRAFT " RÈGLES ", ARTICLE 18, SECTION 2, AND ARTICLE 13, SECTION 4(a) : THE NATURE OF THE TYPE OF A TAXON OF THE FAMILY-GROUP CATEGORY

(Commission Reference : Z.N.(S.) 1280)

(For the relevant provision in the Draft of the Revised *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 117, 97)

DOCUMENT 11/1

Proposed verbal amendment in Article 18, Section 2, of the Revised Draft of the " Règles " and proposed addition of an Explanatory Note to Article 13, Section 4(a) of the above Draft

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Statement received on 25th October 1957)

In 1798 Fabricius established the nominal genus, *Pompilus*. When Leach established the family POMPILIDAE in 1815 this automatically became its nominal type-genus. Authors have supposed that *Pompilus* and POMPILIDAE were spider-hunting wasps, but in recent years it has been discovered that the type-species* of *Pompilus* belongs to another taxonomic family, the thread-waisted wasps. Therefore, the name POMPILIDAE, under the Rules, passed out of the family of spider-hunting wasps and they had to receive some other family name.

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* *Pompilus* and POMPILIDAE have been conserved under the Plenary Powers (1945, *Ops. Decls. int. Comm. zool. Nomencl.* **2** : 375–398). These names are used here only as an illustration, and all statements apply to what would have been the case if the Plenary Powers had not been used.

2. In this case it seemed that a name must be found for the taxonomic genus which hitherto incorrectly had been called *Pompilus*. But what was that genus ? It is impossible to answer that question because only a nominal genus has a type-species, and the group of species which at one time were incorrectly termed " *Pompilus* " have in the course of time been divided into many genera, no one of which has any better claim than another to represent those spider-hunting wasps which were originally, but incorrectly, associated with the name *Pompilus*. However, it has been found that there is a nominal genus to which some of these wasps belong that is even older than *Pompilus*. This is *Psammochares* Latreille, 1796, a name that fell into complete disuse. This taxonomic genus had been renamed *Anoplius* by Dufour in 1834. Banks, 1910, thought that in *Psammochares* he had found the answer to the unanswerable question of a replacement name for the undefinable taxonomic genus that had incorrectly passed under the name *Pompilus*. In reality he had merely uncovered a senior subjective synonym of *Anoplius* Dufour, 1834. Nevertheless, Banks fell into the error of assuming that *Psammochares* must now replace *Pompilus* as type-genus of the taxonomic family of spider-hunting wasps. This is the whole point of what I have been leading up to, for it serves as an excellent illustration of erroneous thinking into which many taxonomists have fallen and continue to fall. Banks, 1910, thought that he was proposing PSAMMOCHARIDAE as a substitute name for POMPILIDAE*. What he actually did, from the view-point of nomenclature, was to establish a new nominal family, dating from 1910, with *Psammochares* as its nominal type-genus. This family name could only compete with other potential family names, as to becoming the correct name for the spider-hunting wasps.

3. The *Règles* provide that the Law of Priority shall govern the names of taxa of the family-group (*Copenhagen Decisions zool. Nomencl.* : 33, Decision 45). APORIDAE Leach, 1815 (type-genus *Aporus* Spinola, 1808, a true genus of spider-hunting wasps) was the first taxon of the family-group category erected within the limits of that family as taxonomists now conceive it. Therefore, APORIDAE, not PSAMMOCHARIDAE is the correct name under the *Règles* (barring action under the Plenary Powers) for the taxonomic family formerly incorrectly known as POMPILIDAE

4. Because the situation illustrated by the preceding example has not always been fully understood and confusion has sometimes arisen, it might be well to slightly amend the *Règles* and to add an explanation to that part of them that deals with changes in names of the family-group.

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* Banks did not know that *Pompilus* was a valid name for a group within another family of wasps, but abandoned the name under the mistaken belief that it was a preoccupied name. This, however, makes no difference for the purpose of my illustration.

5. Article 4 of the existing *Règles* might be divided by preceding the present paragraph with one reading :

> The type of each taxon of the family-group, or higher, shall be a nominal genus.

6. In the revised Draft of the *Règles*[1] the preceding provision would replace Article 18, Section 2.

7. It is quite impossible to make a taxonomic, as opposed to a nominal, genus the type, because the former is purely subjective and incapable of objective definition, except in terms of nominal genera.

8. The following explanation of Copenhagen Decision 54(1)(a) (page 36) might be attached to Article 13, Section 4(a) of the revised Draft of the *Règles*.[2]

> When the type-species of the nominal genus that is the type of a nominal family is found to belong to a taxonomic family other than that to which it had been supposed to belong, the family name passes out of the taxonomic family to which it had mistakenly been supposed to apply and comes to compete in priority with the current name of the family to which it correctly applies. This leaves the misnamed taxonomic family without a name, unless some taxon within that family, such as subfamily or tribe, has already been named, in which case the oldest such name becomes the name of the taxonomic family. All of this applies equally to any taxon of the family-group.
>
> All of the preceding follows logically and inevitably from the provision that names of taxa of the family-group are subject to priority.
>
> If the incorrectly used name of a taxon of the family-group is of some importance and has long established usage, the best course may be to request the Commission to conserve it, under the Plenary Powers. This can be done by arbitrarily establishing a type-species for the type-genus of the family that is in harmony with the customary usage of the name.
>
> *Example* : *Pompilus* Fabricius, 1798, was incorrectly used as though *pulcher* Linnaeus, a spider-hunting wasp, were its type-species, and as such became type-genus of the family POMPILIDAE, 1815. This family name was in use for more than a century. Under the *Règles* the species *viaticus* Linnaeus, a thread-waisted wasp, was the correct type-species, so that POMPILIDAE competed with SPHEGIDAE as the correct name for the thread-waisted wasps. Under its Plenary Powers the Commission designated *pulcher* to be the type-species of *Pompilus*, therefore restoring that genus and the family POMPILIDAE to their customary usage.

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[1] See Vol. **14** : 117.

[2] See vol. **14** : 97–98.

CASE No. 12

DRAFT " RÈGLES ", ARTICLE 11, SECTION 1 : EMENDATION OF GENERIC AND SPECIFIC NAMES

(Commission Reference : Z.N.(S.) 1286)

(For the relevant provision in the Draft of the Revised *Règles* see, 1957, *Bull. zool. Nomencl.* **14** : 88)

DOCUMENT 12/1

Proposed clarification of the expression " evidence in the original publication as used in relation to the emendation of generic and specific names in Article 11, Section 1 of the Draft of the Revised " Règles "

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

Note dated 5th December 1957

The present note is concerned to suggest a slight clarification of the decision (Decision 71(1)(i)(a)) by the Fourteenth International Congress of Zoology, Copenhagen, 1953 (1953, *Copenhagen Decisions zool. Nomencl.* : 43) that the original spelling of a zoological name is the Valid Original Spelling, unless there is clear evidence " in the original publication " that that spelling was the result of an inadvertent error such as a *lapsus calami* or a copyist's or printer's error.

2. This Rule is clear and unambiguous in the case of a name published in a separate work, when issued in a single instalment, but is not free from doubt in the case of a name published in a work issued in instalments or in a serial publication. In the case of a separate work the author or editor by inserting a " Corrigendum " at the end of the volume is able to correct the spelling of any new name included in that volume which had there appeared in an incorrect

form and is thus able to provide " in the original publication " the " clear evidence " that the " Original Spelling " for the name in question was an " Invalid Original Spelling ". In such a case therefore the name in question may be emended to its correct form, the conditions laid down by the foregoing Copenhagen Decision being fully satisfied.

3. The position however is not so clear in the case of a name published in a Part of a serial publication. For (1) the author of a name published in this way has no means himself of providing evidence that an incorrect spelling used for a particular name in his paper is due to any of the special causes specified in the Copenhagen Decision quoted above, and (2) it is the practice of editors to defer the publication of Corrigenda until the close of the volume as a whole. In the absence of some qualification of the wording employed by the Copenhagen Congress in this matter it is questionable whether the publication of a correction in a Corrigenda issued on the completion of a volume of a serial publication can properly be regarded as being made " in the original publication ", since some months at least may elapse between the publication of an incorrectly spelled new name in a part of a volume of a serial publication and the publication of a correction of that mistake in the concluding part of the volume concerned.

4. This point was not expressly raised in the discussions at Copenhagen, but it appears to me that it would be in harmony with the general intention of Decision 71 of that Congress that in the case of a name published in a serial publication, a correction of the spelling so used for that name, if published in a " Corrigendum " included in the final part of the volume concerned, were to be treated as having been published " in the original publication " for the purposes of the foregoing Decision. Any other decision would produce the highly anomalous result that, while misspellings of new names can be corrected in the case of a name introduced in a separate work, if published as a single unit and not in instalments (through the inclusion of a " Corrigendum " at the close of that volume), no corresponding correction could be made in the case of a name introduced in a serial publication or in a separate work if published in instalments (by reason of the fact that in the case of publications issued serially " Corrigenda " are not normally compiled by editors until the close of the volume and therefore normally appear only in the concluding part of the volume concerned).

5. It is accordingly proposed that the foregoing question should be clarified by the addition of appropriate words at the point in the Revised *Règles* where the foregoing decision by the Copenhagen Congress is incorporated. It is recommended also that the same opportunity should be taken to correct another small defect in Copenhagen Decision 71. This is in connection with the use of the word " *lapsus* ". In Article 19 of the existing *Règles* this word appears as part of the phrase " *lapsus calami* " and, so used, makes sense, having the meaning " lapse [or slip] of the pen ". For some reason the word " *calami* " was omitted at the time when Copenhagen Decision 71 was drafted. Accordingly, this portion of that Decision reads " an inadvertent error such as a *lapsus* ". The word " *lapsus* ", when not employed in its principal meaning (a fall, a slipping, a sliding, etc.) has the meaning " a failing ", " an error " or " a fault ", but such errors or faults may be of any kind, the meaning not being confined to slips of the pen, a sense in which indeed the word does not appear to have been used at all in classical Latin. Accordingly, the expression " an inadvertent error such as a *lapsus* " as used in the above Copenhagen Decision is purely tautological meaning only " an inadvertent error such as a mistake ". It is recommended that this matter be rectified by the reinstatement of the word " *calami* " after the word " *lapsus* ".

6. The Copenhagen Decision discussed above appears in Article 11(1)(a) of the Draft of the Revised *Règles* (1957, *Bull. zool. Nomencl.* **14** : 88). It is recommended that the points indicated above be met by the insertion of the following verbal amendments in the foregoing passage :—

(1) Line 6 : After the words " original publication " insert the words " including any corrigendum published for the volume in question " ;

(2) Line 8 : After the word " *lapsus* " insert the word " *calami* ".

CASE No. 13

DRAFT " RÈGLES ", ARTICLE 28, SECTION 13 : SPECIFIC NAMES IN ADJECTIVAL FORM CONSISTING OF PARTIALLY LATINISED WORDS : PROPOSAL THAT SUCH NAMES SHOULD BE TREATED AS " BARBAROUS " WORDS AND THEREFORE AS BEING EXEMPT FROM CHANGE IN GENDER

(Commission Reference : Z.N.(S.) 1064)

(For the relevant provision in the Draft of the Revised *Règles* see, 1957, *Bull. zool. Nomencl.* **14** : 220–221)

DOCUMENT 13/1

Question of the treatment in the matter of liability to gender changes to be accorded to adjectival specific names consisting of partially Latinised words

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 10th December 1957)

In the early part of last year a communication was received in the Office of the Commission from Dr. Leo Sheljuzhko (*Zoologische Sammlung des Bayerischen Staates, München*) on the question of the treatment in the matter of liability to gender changes which should be accorded to adjectival specific names consisting of partially latinised words. The example actually cited by Dr. Sheljuzhko was the name *Parnassius mnemosyne* Linnaeus, form *melaina* Honrath, 1885. The question so raised was whether, having regard to the fact that the generic name *Parnassius* is masculine in gender the adjectival name *melaina* which is feminine in form should be changed to the masculine form and, if so, what would be the correct form for this name.

2. As the result of correspondence with Dr. Sheljuzhko and Professor the Rev. L. W. Grensted, Consulting Classical Adviser to the Commission, I prepared a paper for the consideration of the Commission in which I suggested that the Commission should adopt a *Declaration* that a specific name consisting of a partially latinised adjective should be treated as being a " barbarous " word and should therefore be exempt from gender change. The very interesting Report received from Professor Grensted was attached to the above paper as an Appendix. The paper described above was published on 26th August 1957 (*Bull. zool. Nomencl.* **13** : 235—239).

3. Following the publication of this paper I received a letter dated 17th September 1957 from Professor Ernst Mayr intimating that, in his view, the solution recommended in the above paper was the realistic solution, but adding that in the case of ornithology the practice had been to change the gender of such names to agree with that of the name of the genus in which the species concerned was currently placed, with the result that a given name might appear in the literature as *melas*, *melaena* or *melan* according to the gender of the generic name employed. In reply, I put forward (in a letter dated 21st November 1957) the suggestion that in the circumstances the best course might be for the Commission to adopt a *Declaration* in the sense recommended but should add a rider to it to the effect that, where prior to the proposed Ruling a name of this type had been altered in form to correspond with the gender of the generic name with which it has been combined and that change has become generally accepted, the original spelling should not be restored without prior reference to the Commission. In a further letter (dated 27th November 1957) Professor Mayr expressed doubts as to the practicability of freezing a given ending in a case of the kind under consideration.

4. In the circumstances, I have come to the conclusion that the best course will be for the present matter to be placed on the Agenda Paper for the London Congress rather than that it should be dealt with by way of *Declaration*. I have so informed Professor Mayr.

5. My original proposal for the adoption of a *Declaration* is attached to the present paper as an Annexe. Extracts from my subsequent correspondence with Professor Mayr are reproduced in the immediately following paper, Document 13/2.

ANNEXE TO DOCUMENT 13/1

Proposed adoption of a "Declaration" on the question whether adjectival specific names consisting of not fully Latinised words should be treated, under Article 14 of the "Règles", as consisting of "barbarous" words and therefore as being exempt from change in gender

The purpose of the present application is to place before the International Commission on Zoological Nomenclature certain difficulties which have come to light in connection with the spelling to be adopted for specific names consisting of direct transliterations of Greek adjectives and to invite the International Commission to provide a solution by rendering a *Declaration* clarifying the action which under Article 14 of the *Règles* should be taken in such cases.

2. This problem was first brought to the attention of the Office of the Commission by Dr. Leo Sheljuzhko (*Zoologische Sammlung des Bayerischen Staates*, *München*, *Germany*) who in a letter dated 9th December 1955 enquired what was the correct form for the specific name *melaina* if placed in a genus having a name which was masculine in gender. The word "melaina" is a direct transliteration of the feminine form of a Greek adjective, a direct transliteration of the masculine of which is "melas" and of the neuter "melan". The question for consideration is how a specific or subspecific name consisting of the word "melaina" should be formed when combined with a generic name consisting of a word having either a masculine or a neuter gender.

3. In a case such as that discussed above there are broadly two alternatives: either a specific name such as *melaina* should take the form *melas* if placed in a genus having a name of masculine gender such as *Parnassius* (the genus to which the taxon bearing the above name cited by Dr. Sheljuzhko is currently referred) or (b) such a name should be treated as not being subject to change when the taxon bearing that name is placed in a genus having a name which is either masculine or neuter in gender.

4. As a preliminary to the further consideration of the present problem, I invited Professor the Rev. L. W. Grensted, Consulting Classical Adviser to the International Commission, to examine and report on the problems involved. The very interesting Report subsequently furnished by Professor Grensted is attached to the present note as an Appendix. It will be seen from Professor Grensted's Report that he takes the view that the best course will be to treat specific names of the class discussed above as consisting (in the terminology of

the *Règles*) of "barbarous" words and therefore as being exempt from the normal rules regarding the agreement in gender of adjectival specific names with the generic names with which they are combined. It will be seen also that in his Report Professor Grensted raises also the question of the treatment to be accorded to specific names belonging to a somewhat analogous group, namely names consisting of compound words which are adjectival in form in cases where the final component of the name is wholly Greek in form.

5. Professor Grensted's proposals appear to me to merit full support, for in addition to being logical and self-consistent, they provide a solution which is in harmony with current usage, the adoption of which would avoid unnecessary and undesirable name-changing.

6. If provision in regard to this matter is to be made in the revised text of the *Règles* by the Fifteenth International Congress of Zoology when it meets in London next year, it is clearly desirable that the present problem should be thrown open to general discussion as soon as possible. It is for the purpose of providing a basis for such a discussion that, in agreement with Professor Grensted, I now submit for consideration the proposal that the International Commission on Zoological Nomenclature should adopt a *Declaration* clarifying as follows the provisions of Article 14 in the above regard :—

Draft Declaration

(1) Where a specific name consists of a word which is an adjective in Greek or in any other language, except Latin, and where the exact form of that adjective is retained when the word in question is published as a zoological name, the name is to be treated as being composed of a "barbarous" word and accordingly is not to be subject to change in termination if the specific name consisting of that word is combined with a generic name having a gender different from that in which the specific name in question was cited at the time when it was first published.

> *Example* : A specific name consisting of the word "*melaina*", that word being an exact transliteration of the feminine form of a Greek adjective, the transliteration of the masculine of which is "melas", is to retain the form in which it was originally published, irrespective of the gender of any generic name with which it may be combined.

(2) The Rule prescribed in (1) above is to be applied also to any specific name consisting of a compound word where that word is adjectival in form and its final component is wholly Greek in form and origin.

> *Example* : A specific name consisting of the compound word "*celebrachys*", being a word which is adjectival in form, its final component

being wholly Greek in form and origin, is not to be changed to "*celebracheia*" if the taxon so named is placed in a genus having a name of feminine gender but is to retain its original form, irrespective of the gender of any generic name with which it may be combined.

APPENDIX

On the application of the Rule of Gender Agreement in the case of specific names which are adjectival but have not been Latinised

By **L. W. GRENSTED**, M.A., D.D.

(*Consulting Classical Adviser to the International Commission on Zoological Nomenclature*)

Dr. Sheljuzhko, in a letter to the Secretary of the Commission, has raised a question as to the correct form of the sub-specific name in the case of *Parnassius mnemosyne melaina*. The name *melaina* was first used by von Honrath as the name for an aberration, but later von Bryk raised the form to the status of a sub-species, and accordingly gave *melaina* its masculine form in Greek, *melas*. This raises a question involving a number of specific names which are Greek in form and which have sometimes been attached to generic names with a curious disregard for agreement in gender. Specific names, under the Rules, are regarded as Latin. Should they, when wholly Greek in form, follow the laws of Greek grammar, or not ?

2. The common Greek adjective *melas*, *melaina*, *melan* is a case in point. *Melas* is only found in classical Latin as a proper name, and the feminine *melaina* is not found at all. In Souter's *Glossary of Later Latin* an obscure writer of 4th century A.D. is cited as giving the Latinised *melas*, *melaena*, *melan*, but this is nothing more than a transliteration of the genders of a common Greek adjective. It does not amount to its use. But it enables us to treat *melaena* as a proper Latin transcription of μέλαινα, a fact which may be of service in circumstances which might arise. In zoological nomenclature the use of *melas* begins with an anomaly, never challenged down to the present day, in *Erebia melas* Herbst, where *Erebia* is feminine and *melas* masculine. But *melas* in this case is doubtlessly the name *Melas*, taken from classical mythology, as with so many butterflies, and not the adjective. For *melaina* we have *Sitta melaina* Beseke (1787), and twenty years later, *melaena* appears in *Haltica melaena* Illiger (1807), followed by *Atherix melaena* Hoffmansegg (1820), *Mordella melaena* Germar (1824), *Baris melaena* Germar (1826), *Membracis melaena* Germar (1835), *Cetonia melaena* McCleay (1838), *Locusta*

melaena de Haan (1842), etc. As all these names are feminine no question arises, and none can arise unless one of these species is transferred to a masculine genus, when the question raised by Dr. Sheljuzhko would have to be asked again.

3. *Melas* and *melaina* are purely Greek in form. It is more difficult to know how to regard *melaena*. One solution is to treat it as a neo-Latin adjective in spite of its obviously Greek origin. This solution appears in such regrettable forms as *Abramis melaenus* Agassiz (1835), *Aradis melaenus* Germar (1840), and *Sphaeridium melaenum* Germar (1824). Obviously Germar, who uses the specific name five times, treated it in that way. The alternative is to treat *melaena*, with *melaina*, as a fixed form, not varying in gender. The case can fairly be argued on either side.

4. In this connection the use of the very common Greek adjective micros-a-on is illuminating. This appears as a specific name with a complete and undisputed disregard of gender. Thus *micros*, used correctly with *Tachys*, *Trechus* and *Miarus*, appears in *Bembidium micros* (Sturm) C. R. Sahlberg, *Diss. Ent. Ins. Fenn.* 205 (1827), where the neuter *micron* would naturally be expected. Still more curious are *Bulimus micra* d'Orb, 1837, and *Obeliscus micra* H. Beck, 1837, where the feminine is doubtless based on *Helix micra* d'Orb, 1835. The danger here is that *micra*, like *melaena*, might come to be regarded as a Latin feminine, giving rise to a masculine *micrus*, a form which is wholly unclassical. The obvious suggestion is that an original *micros* or *micra* should remain unchanged, whatever may be the gender of any generic name under which the species concerned may come to be placed.

5. To confirm this we have *Metallina lampros* Herbst, where *Metallina* is feminine, for which the accepted name now is *Bembidion lampros*, where *Bembidion* is neuter, *lampros* being the masculine form of the Greek adjective.

6. In such a specific name as *Eulophus myodus* Walker the difficulty does not arise, for though *myodes* (μυώδης) is a purely Greek form it would not vary at all in any gender when put into Latin lettering.

7. The natural suggestion, upon this evidence, is that where a specific name is wholly Greek in form (or, indeed, of any other language than Latin) it should be treated as " barbarous " and not be subject to any change of gender, even when there is a change of gender in the generic name to which it is attached. This rule might apply when the diphthong ai is transliterated ae, thus covering such cases as *melaena*, but there is an arguable case for treating melaenus-a-um as a neo-Latin adjective.

8. Such a form as *melaneus*, *melanea*, which has been used (as in *Saxicola melanea* Rueppell) is, of course, a true neo-Latin adjective, and would not come under such a rule.

9. The above rule should also apply in compound specific names where the final component of the name is wholly Greek and cannot naturally be given a Latin change of gender. Such names are rare, but a form such as *celebrachys* (*Eustrigiphilus celebrachys* Nitsch in Denny, 1842) would have as its natural Greek feminine *celebracheia*. It would be far better to keep the original form of the name unchanged.

10. There is one group of Greek adjectives which might perhaps be held to constitute an exception to the principle. These are compound two-termination adjectives ending in -os. These do not change in the feminine, but end in -on in the neuter, and were often taken over in their Greek form by Latin writers with a taste for Greek, such as Petronius and Pliny. Thus we find monochromos -on, monochordos -on, paraphoros -on, and many others. The case has not actually arisen with *Nymphalis polychloros* (L.), but *polychloros*, though not classical in either Greek or Latin, is clearly an adjective of this type, and there would be strong classical precedent for writing *polychloron* if the species were ever placed under a generic name of neuter gender. The case is a most unlikely one, and such specific names appear to be extremely rare, if indeed there is another to be found. Probably the best course would be to treat this case too as coming under the Rule suggested above.

11. This note is not intended to be a complete survey of the cases that may arise. Actually it is a very difficult matter to cover the field, since an Index of specific names does not afford a ready clue to their termination. It would be very desirable, before a final decision is taken in this matter, that comments should be obtained from experts in different parts of the field. It would be particularly helpful if specialists who may be aware of analogous cases which would not fall within the scope of the rule suggested above would furnish particulars of those cases, so that the rule might be expanded to such extent as may be necessary.

DOCUMENT 13/2

Correspondence as to the applicability of the gender rules to adjectival specific names consisting of partly Latinised words between FRANCIS HEMMING (Secretary to the Commission) and ERNST MAYR

(a) Extract from a letter dated 17th September 1957 from Professor Ernst Mayr

I am very much interested in your proposed *Declaration* dealing with the action to be taken with respect to the gender of Greek adjectives.

The realistic solution is, no doubt, the one proposed by you even though in the ornith. literature it has been customary to adjust the gender, for instance : *Lanius melas* became *Campephaga melaena* when transferred to *Cambephaga*, *Edolsoma melan*, and finally *Coracina melaena*. There seem to have been enough Greek scholars among the ornithologists to swing eventually the opinion of those who did not know Greek. I do think such adjustments of the gender are much more frequent than is implied in your comments. I suggest you go slow on this, or else there might have to be a lot of changing of well established endings. The disadvantage of the proposal is that one will have to check in what genus each specific name was first proposed !

(b) Extract from a letter dated 21st November 1957 from Francis Hemming

In the circumstances, I have been wondering whether the best thing to do might be for the Commission to take a decision on the lines indicated in my paper but to add to it a rider to the effect that, where prior to the proposed Ruling a name of this class has been altered in form to correspond with the gender of the generic name with which it has been combined and that change has become firmly established, the original spelling should not be restored without prior reference to the Commission.

I should be grateful to have your views on this subject.

(c) Extract from a letter dated 27th November 1957 from Ernst Mayr

If our classification were completely mature, one could freeze one ending. However, we have numerous cases where a name, let us say *melas*, was originally proposed in a genus with a masculine gender, subsequently transferred to a genus with feminine gender and subsequently transferred back to a genus with masculine gender. Let us assume that the feminine ending had been in exclusive use for 40 or 50 years, it would nevertheless seem pedantic to have to ask the Commission to permit restoring the original masculine ending.

There is an alternate solution which might possibly cause fewer difficulties. This would be to consider the adjective *-melas*, *-melaena*, *-melan* as " Latinized " and included specifically as an appendix for those not familiar with grammar. There may be a few more such cases but certainly not many. Incidentally another word which should be mentioned somewhere in the rules is the ending *-cola*. First of all it should be pointed out that this is a noun, and secondly that in spite of the terminal " *a* " the gender is masculine (ardiasic first declension).

To go back to your original draft, I wonder how many Greek adjectives have actually been used in nomenclature without complete Latinization. Perhaps the most sensible approach would be to determine this first, because it would give us a clue how often we will run into a problem. I might add that the mere fact that the ending *-melaena* is used instead of *-melaina* indicates clearly that the word has been Latinized. If this is the case, then it would seem improper to treat it as a barbaric word. There are so many aspects to this case that I would like to have advice from several specialists of medieval Latin.

(d) Extract from a letter dated 17th December 1957 from Ernst Mayr

The more I look into the Latinization of Greek words, the more I realize that we have a tiger by the tail. A casual check through the literature reveals that in ornithology it has been quite customary to treat Greek nouns as if they were Latin adjectives ! We have *leucurus*, *Leucura*, *leucurum* ; *chalconotus*, *-ta*, *-tum* ; *chloropterus*, *-ra*, *-rum* ; *cyanocphalus*, *-la*, *-lum* ; *diophthalmus*, *-ma*, *-mum* ; *macrorhinus*, *-na*, *-num* ; *heliosylus*, *-la*, *-lum*. These are just a few examples. The minute we start legislating we are liable to find ourselves in a morass of difficulties. The simplest solution is probably to advise zoologists to disregard the original Greek and to treat any Greek word used in Latin as if it were a Latin adjective, with a few spectacular and well known exceptions. In view of the fact that melas has probably been used quite a few times (see Sherborn) it might be simplest to say that the grammatical forms of *melas* when Latinized are *melas*, *melaena*, *melan*. In addition to leucomelas (*Temminck*, 1835) I have found also the deviant forms in the literature : leucomela and leucomelanos (Latham, 1790).

The case of *Coracina melaena* is about as good a case as you can find because the specific name is used as *melan* when in combination with the generic name *Edolisoma* or as *melas* when in combination with the generic names *Lanius* or *Graucalus*. By far the majority of ornithologists have adjusted the ending of the specific name each time it was shifted into a genus with a different gender.

DOCUMENT 13/3

Support for the proposed adoption of a "Declaration" to treat barbarous words as exempt from change in gender

By **LEO SHELJUZHKO**

(*Zoologische Sammlung des Bayerischen Staates, Munich, Germany*)

(Letter dated 22nd June 1957)

melas/melaina Was meine Meinung betrifft, nach der Sie mich anfragen, so bin ich volkommen mit Ihrer Ansicht einverstanden, dass in solchen Fällen, wie der Vorliegende, keine Änderung der Namen vorgenommen werden soll. Ich glaube nämlich, dass solche Änderungen nur verwirrend wirken müssten.

CASE No. 14

DRAFT "RÉGLES", SUGGESTED NEW ARTICLE BETWEEN ARTICLES 25 AND 26 : THE CODE OF ETHICS : PROPOSED CLARIFICATION AND EXTENSION OF PROVISIONS IN

(Commission Reference : Z.N.(S.) 763)

(For the suggested position in the Draft of the Revised *Régles* see, 1957, *Bull. zool. Nomencl.* **14** : 189)

DOCUMENT 14/1

The "Code of Ethics" : proposals for clarification and extension in certain respects

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 12th December 1957)

Some years ago the late Professor Z. P. Metcalf (*North Carolina State College of Agriculture and Engineering, University of North Carolina, Raleigh, North Carolina, U.S.A.*) drew attention to a practical defect in the "Code of Ethics" adopted by the Ninth International Congress of Zoology, Monaco, 1913. No action was taken on this matter at that time, for it was apparent that the "Code of Ethics" as adopted by the Monaco Congress was incomplete and in need of a thorough review.

2. This matter was re-examined in the early part of the present year and a paper putting forward proposals for the reform of the text of the "Code of Ethics" was prepared. This paper was published on 28th June 1957 (Hemming,

1957, *Bull. zool. Nomencl.* **13** : 171—76). I have since been informed by Professor Chester Bradley (*Cornell University, Ithaca, N.Y., U.S.A.*) that the proposal submitted has his general support. Professor Bradley has, however, suggested various drafting changes.

3. The problem here involved appears to me to be one, the details of which could more conveniently be discussed round the table than by means of a postal vote in the Commission. I have come to the conclusion therefore that, having regard to the fact that there will be an opportunity for such a discussion at the London Congress in July next year, it would be better that this case should be dealt with by inclusion in the London Agenda Paper rather than by way of a *Declaration*, more especially in view of the fact that such a *Declaration* could not in any case be published until shortly before the London Congress. I have accordingly withdrawn the proposal that the Commission should be invited to adopt a *Declaration* in this case and have entered it as Case No. 14 on the London Agenda Paper.

4. In order that members of the London Congress may be aware of the reasons which led to the submission of the proposal, the text of that proposal is annexed as an Appendix to the present note. In a second paper—to which has been allotted the Number Document 14/2—there is submitted a revised draft which has been prepared in consultation between Professor Bradley and myself and which is recommended by both of us.

APPENDIX TO DOCUMENT 14/1

Proposed adoption of a " Declaration " clarifying and extending the provisions of the " Code of Ethics "

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

The purpose of the present paper is to place before the International Commission on Zoological Nomenclature certain considerations relating to the wording and scope of the " Code of Ethics " and to suggest the adoption of a *Declaration* clarifying and in one respect extending the provisions of that Code

I. HISTORICAL BACKGROUND

2. Before setting out the points on which it is suggested that further action is necessary, it may be convenient briefly to recall the origin and purpose of the " Code of Ethics " and its subsequent history. The precise circumstances which led up to the adoption of the Code are not known and cannot now be ascertained, for the papers relating to this matter were included among that portion of the records of the Commission which owing to storage difficulties were destroyed in 1931. All that is known is that at its Session held at Monaco in 1913 the International Commission adopted a Resolution in which the " Code of Ethics " was propounded. The Commission's Resolution on this subject was embodied in its Report to, and was approved by, the Ninth International Congress of Zoology. Thereafter the " Code of Ethics " was published with all editions of the *Règles*, although it was not formally a part of those *Règles*. When in 1943 the " Declarations " Series was inaugurated, the " Code of Ethics " was embodied in *Declaration* 1 (1943, *Ops. Decls. int. Comm. zool. Nomencl.* **1**(A) **:** 1–6).

3. In 1935 the Commission received from Professor Dr. Eduard Handschin, then President of the Schweizerische entomologische Gesellschaft, a proposal prepared by the Verein Entomologia Zurich that certain specified action should be taken by the International Commission in any case where it was satisfied that a given author had repeatedly and deliberately violated the " Code of Ethics ". Particulars of an individual case which, in the opinion of the two Societies, fell in the above class were furnished in the documents then submitted to the Commission. This matter was considered by the Commission at its Session held at Lisbon in September 1935. In the discussion which then ensued the view was unanimously expressed that the Commission was not equipped for undertaking disciplinary functions of the kind which had been suggested and that it was undesirable that it should be asked to undertake duties of this kind. At this meeting, in the absence through ill-health of Dr. C. W. Stiles, I was officiating as Acting Secretary to the Commission and it appeared to me that in recording the foregoing discussion it would not be appropriate to include particulars of the individual case cited in the application submitted in which, in the opinion of the applicant-societies, a particular zoologist had committed breaches of the " Code of Ethics ", having regard to the expressed unwillingness of the Commission to take individual cases into consideration. Accordingly, both in the Official Record of the Proceedings of the Commission at its Lisbon Session (1943, *Bull. zool. Nomencl.* **1 :** 25) and in the *Declaration* (*Declaration* 12 published in 1944, *Ops. Decls. int. Comm. zool. Nomencl.* **2 :** xvii–xxiv) embodying the decision then taken, all reference to this side of the question was deliberately omitted, the record being confined to a recital of the Resolution in which the Commission placed on record its considered opinion that the question whether the " Code of Ethics " had been duly complied with in any given case was not a matter on which it felt authorised to enter.

4. The text of the " Code of Ethics " as embodied in *Declaration* 1 and as clarified in *Declaration* 12 was examined in Paris in 1948 both by the International Commission on Zoological Nomenclature and by the Thirteenth International Congress of Zoology. As a result certain amendments, particulars of which are given in paragraph 6 below, were made in the text of *Declaration* 1, while as regards *Declaration* 12 greater precision was given to the provision prescribing that it was no part of the functions of the International Commission to exercise functions of a disciplinary character in relation to alleged breaches of the " Code of Ethics ". Subject to the amendments so adopted the Paris Congress decided that a provision embodying the " Code of Ethics " should be incorporated in the revised text of the *Règles* which it then agreed should be prepared. The provision which it was then decided to insert in the *Règles* was in the following terms (1950, *Bull. zool. Nomencl.* 4 : 167) :—

> When a worker notices that a generic or subgeneric name or a name of a species, subspecies or infra-subspecific form published as a new name by an author who is alive at the time of the foregoing discovery is invalid by reason of being a homonym and requires to be replaced, the author making such a discovery should notify the author by whom the name in question was published, and, before himself publishing a substitute name, should so far as practicable, give the original author an opportunity of so doing, it being made clear that the observance of the foregoing provision is a matter to be left to the proper feelings of individual workers, it not being part of the duties of the International Commission on Zoological Nomenclature to investigate or pass judgment upon alleged contraventions of this provision.

II. Examination of certain aspects of the text of the " Code of Ethics "

5. Having placed the " Code of Ethics " in its historical setting, we are now in a position to examine certain aspects of the text of that Code which present features which appear to call for consideration. Two problems are involved. The first is concerned with the question of removing from the text a provision which, if strictly observed, might have the unintended result of seriously impeding the necessary correction of errors arising out of the publication of invalid homonyms. The second is concerned with the question of coverage. The intention of the authors of the Code was no more than to provide a means for discouraging irregular practices in the matter of the replacement of invalid homonyms, and the title " Code of Ethics " given to the resolution so adopted was much wider than the resolution itself. Now, however, that the " Code of Ethics " is, under the decision of the Paris Congress, to be incorporated in a special Article in the revised text of the *Règles* it would be illogical to leave that provision in its present incomplete and unbalanced state. These matters are considered separately below.

(a) Proposed removal of an unduly restrictive provision from the portion of the "Code of Ethics" relating to the replacement of invalid homonyms

6. In the form in which it originally read, an author discovering that a given name was an invalid junior homonym of another name was enjoined under the "Code of Ethics" to give the author of the invalid name "ample opportunity" himself to publish a valid substitute name. Nothing was said in the resolution as what should be done if the author discovering the conditions of homonymy between the two names was unable to get into touch with the author of the invalid name nor was any indication given as to what should be regarded as an "ample opportunity" for the purpose of compliance with the "Code". As will be seen from the text of the decision quoted in paragraph 4 above, an attempt was made in Paris to deal with both of these points, as regards the former by inserting the words "so far as practicable" and as regards the latter by deleting the word "ample" before the word "opportunity".

7. While the drafting changes adopted by the Paris Congress undoubtedly constitute an improvement on the original text, neither, in my opinion, is fully satisfactory. As regards the first, it is only in a minority of cases that an indication of the addresses of the authors of papers are given in serial publications and it is often very difficult to ascertain the address of the author of a paper in a serial published in some foreign country or even to be certain whether the author is still alive. Moreover, in existing world conditions, it is not always possible to communicate with zoologists resident in particular countries or, if one does write to them, to be confident that one's letter is duly delivered. Of these difficulties the first is relevant to the question of making a notification under the "Code of Ethics", the second, to the question of the amount of time which should be regarded as constituting an "opportunity" to the author of an invalid name himself to replace it.

8. In view of the fact that we are here concerned with a provision, non-compliance with which lays an author open to the stigma of having offended against professional etiquette, it seems important that the wording of the provision should be such as expressly to absolve from blame an author who publishes a substitute name for an invalid name published by another author if, after making reasonable efforts, he finds it impossible (a) to ascertain whether the author concerned is alive—this being a relevant factor in that the fact that a given name is an invalid homonym may often not be detected until long after the publication of the paper containing the name in question, or (b), if that author is alive, to communicate with him. It is accordingly suggested in the revised text submitted in paragraph 13 below that words dealing with this matter expressly should be substituted for the words "so far as practicable" inserted in the "Code" by the Paris Congress.

9. The existing difficulties in regard to the interpretation of the expression " opportunity " were brought forward vigorously in the following passage included in a letter primarily concerned with another subject which was addressed to the Office of the Commission on 17th April 1953 by the late Professor Z. P. Metcalf (*North Carolina State College of Agriculture and Engineering, University of North Carolina, Raleigh, North Carolina, U.S.A.*) :—

> While I agree with the general purport of the Code of Ethics, in regard to publishing new names to take the place of preoccupied names, there is another side to this situation. When you write to an author three or more times and he makes no reply or when he says he is going to publish a new name for the preoccupied name in the next number of a journal and then three or six years later, having forgotten his previous promise, repeats the same promise, courtesy ceases to be a virtue.

10. The difficulties discussed above are more likely to confront the compilers of large catalogues than any other class of zoologist and it may be confidently concluded that it was as the author of the *General Catalogue of the Hemiptera* that Professor Metcalf had encountered the difficulties in applying the " Code of Ethics " described in the letter quoted above. The point which he makes is, I consider, a valid one and I suggest that it should be met by the insertion in the " Code of Ethics " of a specified period which, after having made the prescribed notification, an author should be required to wait before himself publishing a substitute name for the invalid homonym in question. It is suggested that the period so to be specified should be " one year ". Even in this case there should, however, in equity be a safeguard exonerating from blame an author who publishes a replacement name for some other author's invalid name where this is necessary in order to save the author concerned from being forced to employ in a book or paper already in preparation a name which he has ascertained to be invalid.

(b) Proposed extension of the " Code of Ethics " to include a condemnation of the publication of a name for a new taxon when it is known that another author has arranged to publish a name for the taxon concerned

11. In its present form the " Code of Ethics " contains a condemnation only of the publication of a substitute name without giving the author of the invalid name a chance himself to publish a valid name. Such practice, though reprehensible, are not, however, by any means the most serious of those which it would be reasonable to expect to see condemned in a " Code of Ethics ". In particular, it seems very anomalous that the " Code " does not condemn the publication of a name for a new species by an author when he knows—or has reasonable ground for believing—that another author has already arranged to publish a name for that species. Fortunately, cases of this kind are rare but they do nevertheless occur from time to time and it would seem appropriate that a condemnation of them should be included in the " Code of Ethics " at a time when that " Code " is incorporated into the *Règles*.

(c) Responsibilities of editors in relation to the observance of the " Code of Ethics "

12. In the case of papers containing new names published in serial publications the editor of the serial would not normally himself possess the detailed knowledge required to enable him to satisfy himself that papers published in the serial for which he was responsible did not contain any breaches of the " Code of Ethics ". It seems reasonable therefore that responsibility for the observance of the " Code " should rest with the author of a paper and that responsibility in this matter should not be imposed upon editors, subject to the condition that no editor should wittingly publish a paper which to his own knowledge contained a breach of the " Code of Ethics ".

III. Recommendation

13. For the reasons set out above it is suggested for consideration that the International Commission should render a *Declaration* substituting for the existing text of the " Code of Ethics " the following revised text in which have been embodied the amendments suggested in paragraphs 7 and 9 above and the extensions suggested in paragraphs 10 and 11 :—

Suggested Revised text of the " Code of Ethics "

(1) The following precepts in connection with the procedure to be observed by authors publishing zoological names form collectively a " Code of Ethics " and any wilful failure to observe these precepts constitutes a breach of professional etiquette :—

(a) An author should not publish a name for a new taxon if he knows, or has reasonable ground for believing, that another author has already arranged to publish a name for that taxon.

(b) An author should not publish a name in replacement of an invalid homonym previously published by another author during the lifetime of that author in any case where he is able to ascertain that author's address and where postal and other conditions make it possible to communicate with that author until :—

(i) he has notified the author concerned that the name in question is an invalid homonym and requires replacement ;

(ii) he has allowed a period of one year to elapse after the despatch of the foregoing notification in order to enable the original author himself to replace the invalid name, save where a delay of so long a period would make it necessary for the author by whom the condition of homonymy had been discovered to employ the invalid name in a work to be published within that period.

(2) Editors and others responsible for the publication of zoological papers should avoid publishing any paper which to their knowledge contains a breach of the foregoing precepts.

(3) The observance of the " Code of Ethics " is a matter for the proper feelings of individual zoologists and the International Commission on Zoological Nomenclature is not authorised or empowered to investigate, or pass judgment upon alleged breaches of its provisions.

DOCUMENT 14/2

Revised proposal for the clarification and amplification of the " Code of Ethics " and a proposal regarding the place in which the " Code of Ethics " so revised should be incorporated into the " Règles "

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 1st January 1958)

The present note has a twofold purpose: First to put forward for consideration a revised proposal for the clarification and amplification of the " Code of Ethics " in place of the proposal published in June 1957 and now republished as the Appendix to Document 14/1, this revised proposal having been concerted in consultation between Professor Chester Bradley and myself; Second, to submit for consideration proposals as to the point at which the " Code of Ethics ", as proposed to be revised should be incorporated into the text of the *Règles* which the London Congress is to be invited to approve this year.

I. Revised proposal for the clarification and amplification of the " Code of Ethics "

2. The revised draft of the " Code of Ethics " embodying the proposals for its clarification and amplification now recommended is attached to the present note as an Annexe. Point (1) of this revised draft is substantially the same as one prepared by Professor Chester Bradley in correspondence following the publication of the original proposal, the only difference being that, in order to avoid repetition, a new Point (2) has been added in place of qualifications inserted by Professor Bradley both in (a) and (b).

3. The points numbered (3) and (4) in the present draft are the same as those numbered (2) and (3) in the original draft. Point (2) contains an admonition to the editors of zoological serials to assist by avoiding the publication of any matter which to their knowledge contains a breach of the " Code of Ethics ". Point (3) places on record that the Commission as a body is not concerned with, and is not empowered to take action in connection with, alleged breaches of the " Code of Ethics ".

4. I think that Point (3) raises a matter of importance for I see no reason why editors should be relieved of responsibility for securing compliance with the provisions of the " Code of Ethics ". Point (4) reproduces a decision taken by the Commission at Lisbon when dealing with an individual case—a decision which was later embodied in *Declaration* 12 and in 1948 was incorporated into the *Règles* by the Thirteenth International Congress of Zoology, Paris, 1948. The existence of this provision has, in practice, proved of value in dealing with suggestions in particular cases that the Commission should intervene in the matter of certain alleged breaches of the " Code of Ethics ".

5. The revised draft now submitted has been prepared in consultation with Professor Chester Bradley who has informed me (*in litt.*, 27th December 1957) that he concurs in its terms.

II. Proposal as to the point at which the revised " Code of Ethics " should be incorporated in the revised text of the " Règles "

6. Bound up with the above question is the related question of the position in which the " Code of Ethics " is to appear in the Revised text of the *Règles* to be approved by the International Congress of Zoology this year. The " Code of Ethics " as such does not appear in the Draft of the revised text of the *Règles* having been incorporated, in part only, as Recommendation 3 in Article 24 (1957, *Bull. zool. Nomencl.* **14** : 172—173), where, it will be noted, it is suggested that the decision by the Paris Congress (1950, *Bull. zool. Nomencl.* **4** : 167) that the " Code of Ethics " should be incorporated into the *Règles* as a mandatory provision should be set on one side.

7. In this connection it is necessary to note that, if, as proposed by Professor Bradley and myself in the draft annexed to the present note, the " Code of Ethics " is expanded to cover the deliberate anticipation by one author of the naming of a new taxon already proposed to be named by another author, the present position of the suggested provision in the Draft of the *Règles* (paragraph 5 above) would in any case cease to be appropriate, for the Draft Article in which that provision has been inserted relates only to the question of the Law of Homonymy (with which the " Code of Ethics " is at present alone concerned).

8. On the broader issue involved, it would, I think, be most unfortunate if the Congress were to be asked to approve a proposal (1) which involved the reversal of the decision by the Paris Congress that the " Code of Ethics " should be included in the *Règles* in a separate Article, (2) which dropped altogether the long-established title " Code of Ethics " and (3) down-graded the provisions now included in the " Code of Ethics " to the status of an inconspicuous *Recommandation* included in some Article dealing with a different subject. On general grounds it is undesirable that one Congress should be asked to reverse a decision affecting the text of the *Règles* taken by an earlier Congress unless it is actively desired to reverse the decision (as was the case, for example, at Copenhagen as regards the problem of the " First Reviser " Principle versus the " Page Precedence " Principle) or unless for some other

for example, at Copenhagen as regards the problem of the " First Reviser " Principle versus the " Page Precedence " Principle) or unless for some other

reason there are considered to be strong grounds for so doing. In the present case, no one, it may be assumed, is in favour of withdrawing the condemnation of certain acts specified in the " Code of Ethics ", for acts which were wrong and improper in 1913 are just as wrong and improper if repeated today. Further, the downgrading to the " Code of Ethics " to the status of a *Recommandation* would appear to me to be open to strong objection, for a decision in this sense would certainly imply that in the field of professional conduct the London Congress has been asked to take, and had taken, a laxer view than that taken by its predecessors at Monaco in 1913 and at Paris in 1948. For similar reasons I should deprecate the atomisation of the " Code of Ethics " by the distribution of its provisions among the Articles relating to homonymy and synonymy respectively, for this also could not fail to give the impression that the subject matter of the " Code of Ethics " was treated more lightly by the London Congress than by its predecessors. The recommendation which I desire to put forward for consideration is therefore that, in conformity with the decision of the Paris Congress the " Code of Ethics ", as now proposed to be expanded, should be incorporated in full in a separate Article of the *Règles*. It would be a matter for discussion what would be the most appropriate place for such an Article. I suggest for consideration that it might conveniently be inserted between Articles 25 and 26 of the draft, i.e. on page 189 of the Draft as published in Volume 14 of the *Bulletin of Zoological Nomenclature*.

Recommendations

9. The recommendations which I now submit for consideration are therefore :—

(1) that the " Code of Ethics " should be amended as shown in the Annexe to the present paper ;

(2) that, as decided upon by the Paris Congress of 1948 the " Code of Ethics " should be incorporated in the *Règles* as a mandatory provision, a separate Article being devoted to this purpose.

ANNEXE TO DOCUMENT 14/2

" Code of Ethics " : revised draft including certain proposed clarifications and amplifications

(based upon correspondence between **J. Chester Bradley** and **Francis Hemming**)

The Code of Ethics

(1) The following precepts in connection with the procedure to be observed by authors publishing zoological names form collectively a " Code of Ethics " and any wilful failure to observe these precepts constitutes a breach of professional etiquette [? substitute " conduct " for " etiquette "] :—

(a) A zoologist should not publish a name for new taxon if he (i) knows, or has reasonable grounds for believing, that another zoologist has already decided to publish a name for it or (ii) that a name for that taxon is to be published in a posthumous work of a deceased zoologist, unless there is an urgent need for the establishment of the new nominal taxon concerned, in which case the second zoologist may establish the taxon in question, provided (1) that he first so notifies the other zoologist, if it is possible to communicate with him, and, (2), having done so, waits for a period of at least one year.

(b) During the lifetime of a zoologist who has published an invalid homonym, no other author should publish a name in replacement of it, or intentionally publish a synonym of it, until, if it is possible to communicate with the former zoologist, he has (i) notified that zoologist that the name in question is an invalid homonym and requires replacement, and (ii) has allowed a period of one year thereafter to elapse.

(2) The period specified in (1) above may be reduced if it is necessary for the second zoologist to refer to the taxon before the expiry of one year from the date of his notifying the first zoologist.

(3) Editors and others responsible for the publication of zoological papers should avoid publishing any paper which to their knowledge contains a breach of the " Code of Ethics ".

(4) The observance of the " Code of Ethics " is a matter for the proper feelings of individual zoologists and the Commission is not authorised to investigate or pass judgment upon alleged breaches of its provisions.

CASE No. 15

DRAFT " RÈGLES ", ARTICLE 8, SECTION 2 (COPENHAGEN DECISIONS 123–124) : PROPOSED RE-INSTATEMENT OF THE " PAGE PRECEDENCE PRINCIPLE " IN PLACE OF THE " FIRST REVISER PRINCIPLE " RESTORED BY THE COPENHAGEN CONGRESS

(Commission Reference : Z.N.(S.) 1291)

(For the relevant provision in the Draft of the Revised *Règles* see, 1957, *Bull. zool. Nomencl.* **14** : 71)

Document 15/1

Proposal for the amendment of Article 28 of the existing " Règles " as amended at Copenhagen (1953) so as to give preference to the principle of page priority in the selection of generic and specific names and for other purposes

By **CYRIL F. DOS PASSOS**

(*The American Museum of Natural History, New York, and The Carnegie Museum, Pittsburgh*)

and

ALEXANDER B. KLOTS

(*The College of the City of New York, New York*)

(Enclosure to letter dated 5th December 1957 from Cyril dos Passos)

Introduction

Prior to the Thirteenth International Congress of Zoology held at Paris in 1948, Article 28 of the *Règles* read as follows :

> Article 28.—A genus formed by the union of two or more genera or sub genera takes the oldest valid generic or subgeneric name of it

components. If the names are of the same date, that selected by the first reviser shall stand. The same rule obtains when two or more species or subspecies are united to form a single species or subspecies.

Recommendation.—In absence of any previous revision, the establishment of precedence by the following method is recommended :

(a) A generic name accompanied by specification of a type has precedence over a name without such specification. If all or none of the genera have types specified, that generic name takes precedence the diagnosis of which is most pertinent.

(b) A specific name accompanied by both description and figure stands in preference to one accompanied only by a diagnosis or only by a figure.

(c) Other things being equal, that name is to be preferred which stands first in the publication (page precedence).

Prior ruling by the Commission

2. But one *Opinion*, *Opinion* 40, appears to have construed Article 28, and that was back in 1912. That case involved two applications to the Commission that were considered together.

(i) The first was : *Salmo eriox vs. S. trutta* and *S. fario* and it was held that on the basis of the evidence submitted it was not necessary to substitute *eriox*, the earliest name, in place of *fario* and *trutta*, later names, because " . . . all other things are not equal in this case, and it is best to select the most commonly used name, which under the premises is *Salmo fario* ".

(ii) The second case, *Heniochus acuminatus vs. H. macrolepidotus* was decided in accordance with the first reviser principle, because it was found that Cuvier (1817) had in fact acted as the first reviser and had selected *macrolepidotus*.

3. At the time of the rendition of *Opinion* 40, the Commission consisted of 11 members, and it was quite apparent from the statements of some of them that they followed reluctantly the First Reviser rule.

(i) Hartert, Commissioner, said in part :

> " The greatest convenience is undoubtedly page-priority, and as it is the only one which admits no discussion (convenience and common usage being uncertain quantities), it alone must decide. Cuvier, in my opinion, was not a ' first reviser ' or monographer.

> He did not revise nor monograph the South Sea fishes, but only mentioned some in this Règne Animal."

(ii) Jentink, Commissioner, said:

> "*Salmo eriox* is the first published name, like also *Chaetodon acuminatus*, and they have therefore priority."

(iii) Jordan (D.S.), Commissioner, said:

> "I personally much prefer the recognition of line and page priority as giving absolute fixity. But I agree that the above is the rule and shall abide by it."

4. Stejneger alone dissented from the *Opinion* upon the ground that Collett united these species in 1875 and that he selected *eriox* as the collective name.

Proceedings at the Thirteenth and Fourteenth International Congresses of Zoology

5. At the Thirteenth International Congress of Zoology the First Reviser rule was abolished, because of dissatisfaction with it, in favour of Page Priority. Although the *Règles* as amended at Paris were never published[1], as was ordered by that Congress, we can obtain the gist of the amendment of Article 28 by referring to Volume 4 of the *Bulletin of Zoological Nomenclature*. There on pages 330–331 we find the following:

> THE COMMISSION agreed to recommend .—
>
> (1) that in place of the provisions in Article 28 relating to names of the same date there be inserted in the *Règles* at an appropriate point provisions (a) that where two or more names were published for the same taxonomic unit, or where the same name was published

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[1] In accordance with the decision here referred to taken by the Thirteenth International Congress of Zoology, Paris, 1948, a draft of the *Règles* as amended by that Congress, was duly prepared. That draft was not, however, published because by the time that it was available it was already evident that it was likely that the text of the *Règles* would be subject to such extensive further amendment that it would be rendered out of date before it could be formally promulgated. In these circumstances the International Trust for Zoological Nomenclature took the view that it would not be justified in incurring the expenditure involved in printing the draft based upon the Paris decisions, considering that the proper course would be to wait until it was possible to publish a draft embodying the Copenhagen, as well as the Paris, decisions. (intl'd. F.H. 20th December 1957)

for more than one taxonomic unit, in the same book or serial or in the same part of any book or serial and were in consequence of identical date, the name printed on the earlier of the pages concerned is to have precedence over the name or names published on a later page, (b) that, where two or more such names are published on the same page, the name which appears on the line nearest to the top of the page is to have precedence over any name or names which appear lower down that page and (c) that, where two or more such names are printed in the same line, a name appearing earlier, is to have precedence over any name or names appearing later, in the same line;

(2) that the provisions specified in (1) above should be subject to the following conditions:—

(a) that, where the application of the foregoing provision would lead to a change in the name of a taxonomic unit of importance, particularly in the fields of medicine, agriculture, veterinary science or other applied fields in biology or in the teaching of zoology, specialists may apply to the Commission for the use of its Plenary Powers to maintain existing nomenclatorial practice and that, on such an application having been submitted, no change in that practice should be made until the Commission's decision is made known;

(b) that the said International Commission shall give sympathetic consideration to applications for the use of its Plenary Powers for the purpose of stabilising names, when requested to do so in accordance with (a) above;

(3) that the existing *Recommandation* to Article 28, being inconsistent with the provisions now proposed to be inserted in that Article, should be deleted therefrom.

6. At the Fifteenth International Congress of Zoology held at Copenhagen in 1953, Article 28 was again amended so as to restore the First Reviser rule. Again it is impossible to quote the exact language of the Amendment, because this has not been published in either draft or final form. However, by referring to the Copenhagen Decisions (1953), edited by Francis Hemming, we find the following (: 66–67):

Article 28

123. Reinstatement of the " First Reviser " Principle in place of the Principle of " Page Precedence " for determining the relative status of

names published in the same work and on the same date : The Colloquium recommends that the decision taken in Paris in 1948 (1950, *Bull. zool. Nomencl.* **4** : 330–331) to substitute the principle of page, line and word precedence for the principle of the First Reviser should be reversed, the original provisions in Article 28 being reinstated with the addition (1) of the definition of the action to be accepted as the action of a First Reviser recommended in paragraph 124 below, and (2) of the *Recommandation* submitted in paragraph 125 below.

124. Definition of action constituting selection by a " First Reviser " : The Colloquium recommends the addition to Article 28 of the following definition of the action constituting action by a First Reviser :—

> The expression " selection by a First Reviser " is to be rigidly construed, and such a selection is to be deemed to have been effected :—
>
> (a) in the case of generic names, only when an author, after citing two or more such names published in the same book and on the same date, clearly indicates by whatever method, (a) that he is of the opinion that the type species of the nominal genera so named represent the same taxon, and (b) that he is selecting one of the generic names concerned, to the exclusion of the other name or names, to be the name to be used for the genus concerned ;
>
> (b) in the case of specific names, only when an author, after citing two or more such names published in the same book and on the same date, clearly indicates by whatever method, (a) that he is of the opinion that the nominal species so named represent the same taxon, and (b) that he is selecting one of the names concerned, to the exclusion of the other name or names, to be the name to be used for that taxon.

125. " Recommandation " urging authors, when acting as " First Revisers ", other things being equal, to apply the Principle of Page Precedence : The Colloquium recommends the addition to Article 28 of the following *Recommandation* :—

> An author, when acting as a First Reviser in regard to two names for the same taxon published in the same book on the same date, is advised to select, other things being equal, the name which appeared first in the work in question, as judged from the standpoint of page and line precedence.

The case of the names " Hesperia hegon " Scudder, 1863, and " Hesperia samoset " Scudder, 1863

7. The above case illustrates how the return to the first reviser rule can upset long-established usage.

8. In 1863 Scudder named two Hesperiidae on the same page of one of his papers, *Amblyscirtes hegon* having line priority. The fact that these names were synonyms was realized a few years (1868) later by Scudder, who thereupon placed *hegon*, although having line priority, in the synonymy of *samoset*. The use of these names by subsequent authors shows that *samoset* was almost uniformly used until 1917 when Barnes and McDunnough changed the name to *hegon*. This was probably due to the influence on American authors of the Banks and Caudell " Code of Entomological Nomenclature " (1912), because that Code prescribed the principle of Page Priority.

9. In 1955 when Evans completed the publication of his monumental work on the American Hesperiids, he felt obliged to use *samoset* and sink *hegon*, claiming that the *Règles* required that action. Thus a name that had not been used for about forty years, with one exception, came into use again and the principle of stability was upset.

10. The odd thing is that Evans, while correct in his conclusion, founded it on a false premise. He relied upon Scudder's revision of 1872, while Scudder had originally taken similar action in 1868, a reference that Evans did not discover. This shows the difficulty of ascertaining who was the First Reviser. Evans was a taxonomist of wide experience with the literature of the world at his elbow, yet he failed to find the first revision.

11. In a case such as this one, involving a search of the literature for nearly one hundred years, there is every possibility of making a mistake and the search may well consume days, whereas page priority is ascertained by a glance at the original description.

Argument

12. This reversal of the page and line priority principle in favour of the first reviser principle was, in the opinion of the authors, an error, with which the great majority of systematists are not in accord for the following reasons :

(1) Page and line priority are objective. There can seldom be any

difference of opinion concerning the order in which pages of a book or periodical were published. Usually they were numbered. Never in the author's experience has there been any dispute on this subject.

(2) The first reviser principle is highly subjective. Often there is difficulty in determining what was the first revision and who was the first reviser. Even then, serious differences of opinion may arise as to what is a revision and who is a reviser.

(3) It is of vital importance that the *Règles* be objective. The more the *Règles* make mandatory an objective method of solving problems, the fewer will be the disputes that will have to be passed upon by the Commission.

Proposal

13. It is the authors' opinion that when two or more names have been proposed at the same time in the same publication for the same genus or other taxon, the first name published (line or page priority) should prevail, unless to insure greater stability the International Commission on Zoological Nomenclature, for cause shown by application to it, shall otherwise order. No such order should be issued by the Commission until one year shall have expired after the publication of the application in the *Bulletin of Zoological Nomenclature*. Such action by the Congress would reinstate the decision taken at Paris in 1948 and substitute the principle of page, line, and word precedence for the principle of the First Reviser. As a proposed text for such a rule the following wording is suggested :

Article 28

Names of the same date :

(a) that, where two or more names were published for the same taxonomic unit, or where the same name was published for more than one taxonomic unit, in the same book or serial or in the same part of any book or serial and were in consequence of identical date, the name printed on the earlier of the pages concerned is to have precedence over the name or names published on a later page ;

(b) that, where two or more such names are published on the same page, the name which appears on the line nearest to the top of the page is to have precedence over any name or names which appear lower down that page, and

(c) that, where two or more such names are printed on the same line, a name appearing earlier, is to have precedence over any name or names appearing later, on the same line ;

that the provisions specified in (1) above should be subject to the following conditions :

(a) that, where the application of the foregoing provision would lead to a change in the name of a taxonomic unit of importance, particularly in the fields of medicine, agriculture, veterinary science or other applied fields in biology or in the teaching of zoology, specialists may apply to the Commission for the use of its plenary powers to maintain existing nomenclatorial practice and that, on such an application having been submitted, no change in that practice should be made until the Commission's decision is made known ;

(b) that the said International Commission shall give sympathetic consideration to applications for the use of its Plenary Powers for the purpose of stabilizing names, when requested to do so in accordance with (a) above.

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DOCUMENT 15/2

Proposed retention of the " First Reviser " Principle for the purpose of determining the precedence to be accorded to any one of two or more names published in the same work and on the same date

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 20th December 1957)

The purpose of the present note is to advocate the retention of the decision taken by the Copenhagen Congress of 1953 to re-instate the " First Reviser " Principle as the method to be applied in determining the relative precedence to be accorded to any one of two or more names considered to apply to the same taxonomic unit in cases where the competing names in question were published in the same work and on the same date.

2. When this matter was considered in Paris in 1948, I supported the policy then decided upon that the " First Reviser " Principle at that time embodied in Article 28 should be replaced by the " Page Precedence " Principle. I advocated this change at that time because of the much greater simplicity of the " Page Precedence " method and the fact that, once the original of the book or paper in which any two given names were first published was available for inspection, the application of the foregoing principle was absolutely automatic and thus not open to challenge. It appeared to me that these were such important advantages as to make the " Page Precedence " Principle superior to that of the " First Reviser ", more particularly in view of the fact that, as experience had shown, the application of the " First Reviser " Principle could not be relied upon to produce unequivocal results in the absence of a provision in the *Règles* defining the nature of the action required to be taken in order to qualify that action as being that by a " First Reviser ".

3. The decision of the Paris Congress to replace the " First Reviser " Principle in Article 28 by that of " Page Precedence " evoked a large body of criticism, mainly from specialists in the United States, on the ground that the abandonment of the " First Reviser " Principle, far from promoting stability in nomenclature, would lead to extensive disturbances in existing nomenclature and consequent name-changing. This question was accordingly placed on the Copenhagen Agenda Paper as Case No. 38, six documents being submitted in connection with this item. These documents were published in 1953 in Volume 10 of the *Bulletin of Zoological Nomenclature* (: 376–390). To any zoologist not familiar with the back-history of the present problem a study of the papers referred to above will be most rewarding.

4. In the light of the documents referred to above I came to the conclusion before the Copenhagen Congress that the balance of opinion was in favour of a return to the " First Reviser " Principle for determining the relative precedence of names published in the same work and on the same date and I accordingly advocated that course, subject to the insertion in the *Règles* of a definition of the action to be treated as constituting action by a " First Reviser " (*Bull. zool. Nomencl.* **10** : 377). The discussion at Copenhagen showed that opinion was divided on this subject but by a large majority the Colloquium agreed to reinstate the " First Reviser " Principle in place of that of " Page Precedence " which had figured in the *Règles* during the preceding five-year period (1948–1953), this decision being accompanied by a further decision to include in the *Règles* a clear definition of the nature of the action which is to be taken in order to qualify the author of any given work as there acting as a " First Reviser " (1953, *Copenhagen Decisions zool. Nomencl.* : 66–67, Decisions 123 & 124).

5. It seems to me quite clear that, whether the " First Reviser " Principle is retained in this matter or whether that principle is once more displaced by the " Page Precedence " Principle, cases will always be found where the principle enshrined in the *Règles* would, if acted upon, give rise to name-changing of an objectionable kind. Indeed, in view of the fact that neither of these principles has been uniformly applied by zoologists as a whole, the occurrence of such cases is inevitable. What is required therefore is the selection of that one of the competing principles whichever is on balance the least likely to give rise to disturbance of existence practice. On this subject I personally found the representations submitted in connection with Case No. 38 on the Copenhagen Agenda Paper very impressive and reached the conclusion that on balance the " First Reviser " Principle was likely to give rise to fewer anomalies than would that of " Page Precedence ". Under either system individual cases would be bound to arise where the only way of preventing undesirable name-changing would be by the use by the Commission of its Plenary Powers. This is particularly likely in the case of a group such as the Lepidoptera where frequently—though incorrectly—the " Page Precedence " Principle had been applied prior to the acceptance of that principle by the Paris Congress of 1948. A striking example of this kind is provided by the publication by Linnaeus in 1758 of two names (*jurtina* and *janira*) for the two sexes of an extremely common European Satyrid butterfly (The Meadow Brown). In this case the " Page Precedence " Principle gives preference to the name *jurtina* and it is by this name that the species has been known for many years. There is, however, a very old but long-overlooked " First Reviser " selection (by Fabricius, 1787) in favour of the name *janira*. The adoption of this " First Reviser " selection would have caused great disturbance in the literature. For this reason the Commission was recently asked to use its Plenary Powers to give precedence to the name *jurtina* over the name *janira* by setting aside the action taken by Fabricius in 1787 (Hemming, 1956, *Bull. zool. Nomencl.* **12** : 279–286) and the request so made has since been granted by the Commission (in *Opinion* 506, now in the press).

6. I am in complete agreement with my friends Mr. dos Passos and Professor Klots in their desire to prevent unnecessary disturbance in the currently accepted nomenclature but I cannot share their view that in the present context the best solution would be to reinstate the " Page Precedence " Principle at the expense of the " First Reviser " Principle (see Document 15/1), for I consider that the better course for zoological nomenclature as a whole would be to retain the " First Reviser " principle, while tempering any ill-effects to which it might give rise in particular cases by a judicious use by the Commission of its Plenary Powers.

CASE No. 1

(*continued from page* 246)

DOCUMENT 1/44

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Reference : Professor Moore's Digest reproduced as Document 1/15)

By **KLAUS J. MÜLLER**

(*Technische Universität Berlin, Germany*)

(Letter dated 22nd November 1957)

These notes are intended as an opinion on the proposed action of Prof. R. C. Moore and Mr. P. C. Sylvester-Bradley, insertion in the *Règles* of a provision defining and providing for the nomenclature of " Parataxa ".

2. Workers in systematic zoology may be appalled at the increase of names which would result from this provision. On the other hand, insertion of parataxa into the *Règles* could introduce stability in the nomenclature of disjunct elements and thus serve as an aid for comparisons in those groups in which isolated parts exclusively or at least predominately have important applications, e.g., some branches of micropaleontology.

3. Following submittal of a formal application by specialists, it is believed that permission for introduction of parataxa should be granted by the Commission only for those groups where isolated parts have proved to have broad significance, not restricted to taxonomy alone.

4. In order to designate clearly the fundamental difference between taxon and parataxon it is proposed to mark the names of the parataxa in a simple fashion. A preceding " degree " sign or super-script lower case letter " o ", e.g., ^{o}*Prioniodus elegans* Pander, 1856, would indicate that this is a partial component which may or may not be a part of a described animal. (See also Müller, 1956, p. 1327–28).

5. As I am working on conodonts, it may be logical to add observations in regard to these particular microfossils, of still unknown affinity. Ordinarily, single conodonts are preserved as fossils ; assemblages believed to be part of an individual are preserved only under exceptional conditions and to date are known from a few localities only. These assemblages in some cases are composed of as many as 8 widely diverse form-types which have been named as different genera. According to Article 27(a) of the *Règles* an assemblage should receive the name of the first described portion which is included. However, there are many natural assemblages which contain the same form-types of conodonts without being closer related. This is the case, e.g. with *Hindeodella* which was found in three, and *Lonchodina* which was found in two different types of assemblages (Rhodes, 1953).

6. Application of Article 27(a) would produce permanent instability of the names of isolated elements, which alone are applicable in micropaleontology for determination of the age and the environmental conditions during the formation of the rocks containing them. For practical reasons a system for the isolated conodonts is needed which, as far as I can see, only could be attained adequately by introduction of parataxa, as proposed by Moore & Sylvester-Bradley.

7. The assemblages in most cases have already received new names in their original descriptions. In conodonts, there are but two cases in which generic names have been used for a disjunct element as well as for an assemblage :

Polygnathus. The lectogenotype *P. dubius* was erected by Hinde, 1879 for a presumed assemblage. Bryant, 1921, restricted the name *Polygnathus* to a partial-genus and in this meaning it has been used subsequently. This existing usage could be retained, or the partial-genus, as defined by Bryant, will have to receive a new name.

Gnathodus was well established by Pander, 1856, for an isolated element. H. Schmidt, 1934, used this name in accordance with the present *Règles* for an assemblage containing the element. Subsequent authors disregarded this action and it has been used since for isolated elements. Schmidt's assemblage will have to receive a new name.

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DOCUMENT 1/45

Remarks on the value of parataxonomy in the case of the Phylum Porifera

(Commission Reference : Z.N.(S.) 1056)

By **R. E. H. REID**

(*Queen's University, Belfast*)

(Enclosure to letter dated 28th November 1957)

The spicules of which the skeletons of sponges are composed are divisible into two groups according to the degree of connection which exists between them during life.

(1) All spicules are initially separate from one another. Some remain separate permanently, and are only connected together by living tissues or additionally by organic fibres (spongin). As a result they are typically subject to dissociation and dispersal when the sponge dies. This is the case with (i) all spicules in many sponges, (ii) some but not all of the megascleres of certain others, and (iii) normally, all microcleres.

(2) In certain cases spicules are united, either by loose to rigid articulation (zygosis) as in lithistid Demospongia, or by actual fusion together as in various Hexactinellida. The resultant structures do not disintegrate on death of the tissues, unless subjected to mechanical fragmentation. The spicules concerned are usually choanosmal megascleres, but dermal (or gastral) megascleres may also be affected. Skeletons of this type account for the majority of sponge remains found as macrofossils. One obvious potential assessment of taxonomic values is that remains referable to these groups should be treated as respectively suitable for description in terms of parataxonomy and ordinary taxonomic categories. This view, however, seems open to objections.

2. It is certainly true that isolated sponge spicules can provide a case analogous with that of the Conodonts. The spicules concerned are usually megascleres, as microscleres are rarely discovered as fossils. It is, first, not uncommon for megascleric elements to be of several kinds in the same sponge, e.g. hexactins, pentactins and rhabdodiactins in a Hexactinellid, or protriaenes, anatriaenes and oxeas in a Demosponge. Secondly, similar types of spicule may recur repeatedly in widely different sponges, e.g. dermal pentactins in Hexactinellida, dichotriaenes in choristid and lithistid Demospongia. Hence the basing of names on isolated spicules can potentially result in (i) use of different names for individual elements of a single sponge skeleton, and (ii) inclusion under the same name of similar elements from skeletons of different sponges. However, this problem has long been recognised by spongiologists. Schrammen, for instance, has preferred to illustrate (e.g. 1936) isolated elements of indeterminate status without referring them to specific or generic categories, instead listing them simply in terms of the general spicular categories to which they belong and the locality and horizon from which they were obtained. From a zoological, or palaeozoological, viewpoint, I see no reason for regarding the possible use of parataxa in such cases as providing any advantage over the practice cited ; on the contrary, if spicules are not thought sufficiently diagnostic for establishment of a normal species, then nothing of zoological value is achieved by naming them artificially. The viewpoint of stratigraphy is, of course, different, involving the desire for definite names applicable to all distinguishable objects, particularly where these are of stratigraphical value as in the case of Conodonts. I do not think, however, that any comparable need for parataxonomic nomenclature can be said to exist in the case of sponge spicules, considering how little use has been made of these bodies by stratigraphers.

3. On the other hand, it has to be realised that isolated spicules do not invariably lack diagnostic value. For instance, *Hindia pumila* Hinde (1887–1912) is a species based on dissociated megascleres in the form of tripodal desmas. I know no grounds on which it could be claimed that entire examples of the species from which these originated could not be identified if discovered, or for thinking that more than one species is represented. The spirally ornamented megascleres ascribed to *Spiractinella wrightii* (Carter) (Hinde, *ibid.*) provide another example. The reference of such names to a system of parataxa, wholly separate from normal nomenclature, seems quite unjustifiable. On the contrary, if isolated examples of a particular type of spicule can reasonably be expected to provide a basis for identifying the complete species, it seems to me desirable that names should be based on such spicules and be normally available for taxonomic purposes. It also seems fair to point out that occasional spicular types have high diagnostic value, even in isolation. An extreme example is the distinctive subdermal hexaster of the Hexactinellid *Aulocalyx* F. E. Schulze. Hexaster microscleres are not in general diagnostic of any taxon beyond the subclass Hexasterophora F. E. Schulze, but the form known as the rhopalaster is not recorded from any Hexactinellid other than *Aulocalyx*.

4. The case of sponge skeletons in which spicular elements are united by articulation or fusion presents a comparable analogy with that of the ammonite conch. It may at first sight appear attractive to represent megascleric skeletal

structures as providing a generally sufficient basis for the establishment of true generic and specific taxa. This view is certainly superficial. It is true that these structures have a generally high value compared with isolated elements, and that their adequacy for diagnosis is assumed by palaeontologists. The loose spicules (i.e. those which are only connected by the tissues) can in fact be closely similar in forms whose connected skeletons refer them to different families. On the other hand, there are also cases where the species of a genus, e.g. the Hexactinellid *Farrea* Bowerbank, cannot be separated by reference to the skeletal framework, the diagnostic features being provided by the loose spicules. The same can also apply to separation of genera, e.g. *Farrea* and *Claviscopulia* F. E. Schulze. In other words, there are cases where connected skeletal structures are just as much unidentifiable discrete parts as are many isolated spicules. The fact is that diagnostic characters are not inherently associated with any particular division of the skeleton. Hence it does not seem possible to make any rigid generalisation, except (a) on a purely arbitrary basis, or (b) on the basis of contention that the only type of material invariably sufficient for recognition of true generic and specific taxa comprises complete specimens with all spicules and soft parts present. Regarding the possibility of analogy between loose spicular elements and aptychi, I do not think it can be said that any serious problem exists due to named forms of isolated spicule occurring in association with more than one kind of named skeletal framework. In any case, such an association is, surely, a clear indication that type material to which a name based on a discrete spicular element is attached cannot be identified as representing any species based on a skeletal framework. Any nomenclatorial problem resulting from assumption that a species name of the former type must necessarily be a synonym of one of several of the latter is therefore essentially artificial and unrealistic in terms of demonstrable relationships. I do not think it can be claimed that introduction of parataxonomy as a general solution for such cases has any appreciable general advantage over a simple provision for suppression, subject to normal request and approval, of unwanted names which are demonstrably non-diagnostic.

5. It also seems fair to comment in passing that the supposed adequacy or otherwise of particular animal parts for normal taxonomic purposes is essentially subjective. For instance, ammonite conchs are alleged to provide a sufficient basis for use of normal nomenclature, where aptychi do not. In terms of the whole animal, however, the conch is presumably just as much a discrete part as an aptychus plate. One wonders what view might be taken of the conch if soft parts—say, the tentacles and hood, assuming such to have been present—were available for study.

6. Another point which seems to need comment is the different value of discrete sponge remains at different taxonomic levels. For instance, material which is of little value at specific level may be referable with certainty, so far

as is known, to a definite genus. It seems possible that indiscriminate introduction of parataxonomy for application to discrete sponge material could lead to difficulties in this connection. An extreme example was encountered recently by the writer. The extinct Hexactinellid order Hemidiscosa Schrammen (1924, as Hemidiscaria) was established on the basis of dissociated microscleres of a type known as hemidiscs, or hemiamphidiscs (Schrammen, 1924, 1936). To provide this Order with a generic type, I have had to base a new nominal species (the name of which will appear in a paper entitled "A Monograph of the Upper Cretaceous Hexactinellida of Great Britain and Northern Ireland" in the 1957 volume of the Palaeontographical Society's monographs) on one of Schrammen's figured hemidiscs. Now the nearest homologue of the hemidisc in living forms is the amphidisc, seen in all Amphidiscosa. These microscleres are so generally similar among members of that order that a specific and generic name based on an isolated example would in general be worthless. A name based on an isolated hemidisc is open to similar suspicion, at least as a species name, though this is, of course, by analogy rather than direct evidence. Other names could be based on other examples; however, living Amphidiscosa often have amphidiscs of more than one kind, so such names would be open to suspicion for a further reason. Here, one might think, is a case for parataxonomy. On the other hand, it is clear that there must have been at least one true species. Similarly there must have been at least one genus, and no grounds can be cited for distinguishing more than one. At ordinal level, the hemidisc is (a) acceptable evidence of membership of the subclass Amphidiscophora, and (b) distinctive as a spicule not found in any living Amphidiscophoran. Thus what is a dubious basis for establishment of a species has a different perspective at higher taxonomic levels; in the order group, it is evidence of the former existence of Amphidiscophora with a microsclere unknown in living forms, for which a taxonomic distinction at this level needs to be recognised. Now: can it be possible for a genus recognised in normal taxonomy, as in this case one needs to be, to have for its type a species distinguished as a parataxon?

7. With these points in mind, I do not think it is desirable to introduce a system of parataxonomy as the basis for nomenclature of discrete sponge remains in general. I do not imply that grounds might not be put forward for use of parataxa in particular cases. I think, however, that any such usage should be on a critical and selective basis, applied in individual cases judged on their own merits, and subject to the approval of the International Commission on Zoological Nomenclature after due notice and allowance for objection by other specialists. I consider that such usage should be restricted to cases where:

(a) it is desired to suppress, for normal taxonomic purposes, a name based on a discrete skeletal part known to occur in several distinguishable skeletal assemblages, or in association with more than one type of distinguishable connected skeletal structure, which for some reason it is desired to retain for other—e.g. stratigraphical—purposes ; or

(b) it is desired to establish a name for a previously unnamed discrete skeletal part, for purely stratigraphical purposes, from which it is not expected that any single species will be identifiable.

It may have been noticed that no distinction has been made above between zoological and palaeontological viewpoints. This is deliberate. No practical taxonomic difference exists between the status of dissociated sponge spicules from a recent dune sand and from the residue obtained by solution of a Carboniferous limestone ; a macerated skeletal framework dreged from the present Atlantic presents essentially the same problem as a similar framework from the Cenomanian of southern England. Hence I see no grounds for generally different viewpoints.

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DOCUMENT 1/46

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **ARTHUR N. DUSENBURY, Jr.**

(*Creole Petroleum Corporation, Jusepín, Monegas, Venezuela*)

(Letter dated 17th November 1957)

I wish to support the proposals of Professor Raymond C. Moore and Mr. P. C. Sylvester-Bradley that provisions should be inserted in the *Règles* recognising " parataxa " as a special category for the classification and nomenclature of discrete parts or fragments of animals, such as conodonts, which are inadequate for identification of whole-animal taxa, yet are frequently valuable in stratigraphic determinations. The majority of paleontologists will probably be found to favor these proposals with enthusiasm.

2. However, in my opinion and in that of Creole paleontologists whom I have consulted, there is a serious defect of omission in the proposals as submitted. Although Moore and Sylvester-Bradley have carefully separated and distinguished between the two major categories which they call taxa and parataxa, they have failed to provide names to distinguish the individual taxon from the individual parataxon of corresponding rank. Their " species ", " genera " and " families " may refer either to taxa or to parataxa, and it becomes necessary to consult the context to discover, if possible, which is meant. This can only lead to confusion. Furthermore, in spite of the fact that they have demonstrated that the " species ", " genera " and " families " of parataxa are not true species, genera and families, i.e., whole-animal taxa, they nevertheless propose to add these admittedly false " species " to the *Official List of Specific Names in Zoology*, these admittedly false " genera " to the *Official List of Generic Names in Zoology* and these admittedly false " families " to the *Official List of Family-Group Names in Zoology*. This particular proposal seems quite self-contradictory to me, and I would like to see it rejected.

3. Turning from destructive to constructive discussion, I propose that the names " species ", " genus ", " family " etc. be applied only to taxa and that their use for parataxa be prohibited. If such a proposal were adopted, it would next become necessary to choose suitable names for the corresponding parataxa.

4. One possibility would be the general acceptance of the scheme devised by M. Georges Deflandre, according to which the names " *legio* ", " *cohors* ", " *manipulus* " and " *centuria* " would be used for the parataxa corresponding respectively to the taxa known as " order ", " family ", " genus " and " species ". Although at first glance this would appear to be an excellent scheme, the amount of acceptance which it has won is surprisingly small. One reason may be that the organisation of the Roman army is unfamiliar to the average modern zoologist or paleontologist, so that he is unable to recall these names of Deflandre readily and, if he does succeed in memorizing them, is likely to forget whether a *cohors* is composed of *manipuli* or vice versa. Another reason may be that there is no mental association between " *legio* " and " order ", " *cohors* " and " family ", " *manipulus* " and " genus " or " *centuria* " and " species ".

5. On the other hand, the mental association between " taxa " and " parataxa " is excellent and obvious. Unfortunately, " *para-* " is a Greek prefix and ought not to be combined with Latin words such as *species*, *genus* and *familia*. However, the Latin prefix " *pro* ", meaning " for ", " in behalf of " or " instead of ", is available, and we may therefore appropriately coin the words " prospecies ", " prospecific ", " progenus ", " progeneric ", " profamilia " (or " profamily ") and " profamilial ". Although these names are quite artificial, they are easy to remember and easy to associate with the corresponding taxa. If any one has a better idea, I am open to suggestions.

6. If the above names coined for parataxa should be accepted, the next step would be to establish the necessary official lists and indexes for parataxa, viz.:

(1) *Official List of Prospecific Names in Zoology*

(2) *Official List of Progeneric Names in Zoology*

(3) *Official List of Profamilia-Group (or Profamily-Group) Names in Zoology*

(4) *Official Index of Rejected and Invalid Prospecific Names in Zoology*

(5) *Official Index of Rejected and Invalid Progeneric Names in Zoology*

(6) *Official Index of Rejected and Invalid Profamilia-Group (or Profamily-Group) Names in Zoology*

DOCUMENT 1/47

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **F. H. T. RHODES**
(*University College, Swansea*)

(Enclosure to letter dated 29th November 1957)

The recent application by Professor Moore and Mr. Sylvester-Bradley is one which I strongly support. I am especially interested in the establishment of parataxa as an aid to the study of the conodonts, and I wish to consider particularly how this proposal relates to special problems in this fossil group, which Moore and Sylvester-Bradley have considered in their First Supplemental Application.

The Nature of Conodonts

2. The conodonts are an extinct group of tooth-like microfossils, of obscure affinities. They are found in a variety of sedimentary rocks, ranging from Cambrian to Triassic in age, and have a world-wide geographic distribution. They are composed of calcium phosphate, with characteristics resembling those of apatite, and they exhibit considerable diversity in form. The early types tend to be simple conical or blade-like forms, and the later types either blade-like or platform-like. They have been variously assigned to the Gastropoda, Crustacea, Pisces, and Annelida, and there is at present little agreement concerning their zoological affinities (see Rhodes, 1954). They are of the greatest value as index fossils in the classification and correlation of strata.

Existing Nomenclature of Conodonts

3. Conodonts were first described by Pander over a century ago, and since then more than 600 publications have been devoted to them. The lack of knowledge of the zoological affinities and the function of conodonts has raised a number of taxonomic problems. Existing methods of nomenclature are of two types.

(a) Because of the selective processes of fossilisation and the usual procedures for the extraction of microfossils, the great majority of conodonts are known as single, isolated specimens. Most conodont students have followed Pander (1856) in adopting a classification based upon the recognition of various types of these individual conodonts as genera and species. A binomial classification therefore exists, which has been developed by workers who rigidly observed the Code of Zoological Nomenclature. More than 1,800 species have thus been erected. Such taxonomic categories are distinguished hereafter in the present text by referring to the category in quotation marks (" genus ", " species ").

In addition a number of " supra-generic " taxonomic categories have been established (Ulrich and Bassler, 1926 : Branson and Mehl, 1944).

Experience has shown that this method of classification is both a convenient and a useful one. The majority of " species " are well-established and the nature of morphological variation in conodonts is such that the method of classification is readily applicable. A considerable number of " species " have been shown to have a wide geographic distribution, and many are of great value in stratigraphic correlation.

(b) Recent studies (Schmidt, 1930, 1950 : Scott, 1934, 1942 : Eichenberg, 1930 : Du Bois, 1943 : Rhodes, 1952, 1953*a*, 1954) have shown, however, that as many as five of these " genera " of conodonts may be present in a single assemblage, which appears to represent the remains of an individual animal. Recognising that the earlier classification of individual components as different " genera " was therefore invalid, some workers have proposed a new classification, based upon the recognition of conodont assemblages as natural taxonomic units (i.e. as representing the remains of individual organisms).

4. There are three more or less distinct ways in which the nomenclature of natural conodont assemblages have been established.

Method I. Assemblages have been named after the earliest applicable name of any component which they contain (e.g. Eichenberg, 1930 : Schmidt, 1934 : Sinclair, 1954).

Method II. Assemblages have been given new binomina, and the component conodonts have been designated by descriptive technical terms. Scott, 1942, followed essentially this practice, identifying the " genera " (but not the " species ") represented in two genera of natural assemblages, and describing the components by common nouns coined from the " generic " names. Thus specimens of *Hindeodella* were termed hindeodells, etc. Scott emphasised, however, that he considered it desirable that the earlier " form-classification " should be retained (1942, p. 295), even though he found it inconvenient to employ it for assemblages.

Method III. Assemblages have been given new binomina and the component conodonts have been designated by their previously established " generic " and " specific " names (if any). (e.g. Rhodes, 1952.)

The interpretation of conodont assemblages

5. Since the decision as to whether a conodont assemblage is " natural " is subjective, it is pertinent to summarise the evidence upon which this conclusion is based.

(a) The assemblages have been described from both Europe and North America, but they are known only, as yet, from strata of Carboniferous age. All the strata from which they have been described appear to represent very quiet conditions of sedimentation.

(b) At least seven genera of organisms appear to be represented by the known assemblages. Within each of these types of assemblage the component conodont elements are generally constant in the variety of forms represented and in their number, allowing for the vagaries and differential hazards of fossilisation.

(c) The component conodonts of assemblages are usually found to be paired, and are frequently preserved in what appears to have been their original opposed positions. Isolated pairs of conodonts are not infrequently found, as well as those within more complete assemblages.

(d) Such paired components are fundamentally similar to one another in all but the more minute morphological details, except that one may be the mirror-image of the other, representing " left and right " forms.

6. These criteria seem to be sufficiently strong to leave little reasonable doubt that the assemblages described are " natural " (as opposed to coprolitic or fortuitous) associations. Random assemblages are, however, known, and I have elsewhere discussed the problems which they present (Rhodes, 1952).

Morphological variation within natural conodont assemblages, and its importance in taxonomic procedure

7. Moore & Sylvester-Bradley (1957, First Supplemental Application) have considered the technical problems involved in the existing system of nomenclature. The variation in the individual conodont components of natural assemblages is of such a kind that it is an important factor in nomenclatorial considerations. The following summary indicates the main features of this variation.

(1) Natural assemblages contain paired conodonts, which represent from three to five " genera ".

(2) The total number of component conodonts present in known assemblages varies from fourteen to twenty-two.

(3) Some component " genera " are represented by a single pair of conodonts, others by up to four such pairs.

(4) The same component " genera " may be present in more than one type of natural assemblage (i.e. more than one natural genus).

(5) The component conodonts of a natural genus may show appreciable variation in form, many previously described " species " being recognisable within assemblages referred to a single natural taxon. (It is not yet known whether such variation is specific or infraspecific.)

(6) Some indication of the degree of infraspecific variation in natural assemblages may, however, be provided by the extent of variation within a pair of components extracted from the same assemblage (see Rhodes, 1952, pl. 127, figs. 5, 6 and 11, 12). This variation, although slight, is such that some of the more " extreme " conodont students would probably recognise each of the paired individuals as different " species ". Such paired individuals also commonly differ in being " left and right " forms, the one being a mirror-image of the other, but virtually all conodont workers regard such variation as " infra-specific ".

(7) Condont " genera " which are found together in natural assemblages, are frequently not coextensive in their stratigraphic ranges.

Taxonomic procedure

It is necessary now to consider taxonomic procedure :—

(1) If freedom of taxonomic expression is to be granted to those who work both with natural assemblages and to those who work with individual conodonts, the nomenclatorial regulations should satisfy five requirements.

(a) They must provide a method for the recognition and classification of natural conodont assemblages.

(b) They must provide a name to differentiate each of the diagnostic forms of individual conodonts, which are of value in stratigraphy.

(c) Homonymy between these two systems of nomenclature must be avoided.

(d) Both systems must be within the legal framework of the International Code, and must derive the protection, stability and uniformity which the Code provides.

(e) Any changes in procedures should be of such a kind as to produce the minimum possible disturbance in the existing nomenclature.

(2) Of the three methods of dealing with the present taxonomic problem listed in paragraph 4, Method I, in which the assemblages are named after the earliest applicable name of any component conodont which they contain, is the " legal " solution under the existing *Règles* (Article 27). It involves serious difficulties of two main types.

(a) The name to be used for the assemblage. Clearly the name that must be applied to the assemblage is that given to the *first-named* part of the animal. If this is done, the following considerations arise :

(i) Objective identification with a natural genus can only be made if the type species is present in the assemblage.

(ii) One " genus " may be present in more than one type of natural assemblage (i.e. in more than one natural genus).

(iii) It might be suggested that this problem may be overcome by a modified application of the Principle of Priority, so that the name of a " genus " unique to the particular type of natural assemblage is selected. Conodont assemblages are rare, however, and it is quite impossible to predict whether or not any " genus " will prove to be peculiar to a single type of assemblage.

(iv) Conodont specialists find it convenient to distinguish two " genera ", *Streptognathodus* and *Idiognathodus* on minor morphological features, in spite of the fact that the two genera are transitional*. Rhodes (1952, 1953) has shown, however, that *Scottognathus*, a genus represented by " natural " conodont assemblages, may contain either one or other of these genera, which are transitional within the assemblages. Similar cases may also exist, and it would be misleading if one of these " generic " names were applied to assemblages in which the " genus " itself was not present. It may be argued that the " genera ", if transitional, must *ipso facto* be synonymous, but practising palaeontologists would reserve the right to dispute this principle.

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* This neontologically offensive procedure is not uncommon in palaeontology, for chronological fossil sequences show all grades of transition. Ultimately taxonomic units are more or less arbitrary subdivisions of more or less continuous faunal sequences, although often such taxonomic division appears to be " natural "—that is, it corresponds with " natural " faunal breaks, which result from such factors as sedimentary hiatus, migration, etc.

(v) Similar problems to these three noted above, arise in the choice of a specific name. Other aspects of the problem of the choice of a specific name have been discussed by Moore and Sylvester-Bradley.

(b) The name to be applied to isolated conodonts.

(i) If the earliest applicable name of a contained conodont component should be applied to an assemblage, all other " species " identified within that assemblage would be junior synonyms of that name. This would result in utter confusion in the nomenclature of the isolated conodonts. Some name must be available to designate variation in isolated conodonts, which are of great stratigraphic importance.

(ii) Some writers (e.g. Sinclair, 1953, p. 489) have argued that, if this method were adopted, it would be possible to designate individual conodonts as (for example) the " subbryantod element of *Streptognathodus elegans* " (where the binomen is that given to a natural assemblage). The term " subbryantod " is coined from the " genus " *Subbryantodus*. This might appear satisfactory for conodont components which show little variation in natural assemblages. Some elements, however, are present in more than one genus—so that two or more names are applicable. This results in three possible " states of synonymy "—at three categorical levels. In any given case all three might apply. Thus two " species " *Xognathus aa* and *Xognathus ba* of isolated conodonts may be regarded as synonymous. Both these might be further shown to be present in the same assemblage, with the selected name of which at least one or possibly both would then be synonymous. But thirdly, they might also be found to be present in more than one type of natural assemblage : in which case the descriptions " xognathid element of *Alpha Xognathus beta* " and " xognathid element of *Gamma Xognathus delta* " would be synonymous at a third (quite different) taxonomic level ! One need not elaborate the taxonomic confusion that would result from such a situation.

(iii) But this would be only the beginning of the confusion, for only those isolated conodonts which show little variation in natural assemblages have been considered. In many cases, however, this variation is considerable, and a " specific " qualification would be necessary to designate any particular form (e.g. the subbryantod type 23 element of *Streptognathodus elegantulus*). This would not only involve a complete revision of conodont terminology and the substitution of a clumsy and unsatisfactory system of nomenclature for that which at present exists, but it would also deprive the new system of nomenclature of the uniformity and protection which the *Règles* afford.

(iv) Even if, in spite of this, this solution were to be accepted, one insurmountable problem would remain. Only a very few " genera and species " (perhaps fewer than 5 per cent. of the " species ") are at present known as components of natural assemblages. For the great majority of isolated conodonts, therefore, *no* name would be available.

(3) It may be suggested that to avoid this confusion, all conodont " genera " and " species " not based upon natural assemblages should be regarded as invalid. Names should be applied only to assemblages, and suitable technical terms should be used to designate isolated component conodonts (Method II of paragraph 4). Such a solution would admittedly reduce the problems of synonymy involved, but all the other major problems discussed above would remain.

(4) The third possible method would be to give new names to natural conodont assemblages, and to retain the existing system of nomenclature for isolated conodonts. (Method III of paragraph 4.)

8. A consideration of the factors listed above, will indicate that this represents the best possible solution. It is illegal under the existing *Règles*, however, but it would be regularized by the recognition of parataxa. Only by this method can the problem confronting conodont workers be satisfactorily resolved.

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DOCUMENT 1/48

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **ELLIS L. YOCHELSON**

(*Washington, D.C., U.S.A.*)

(Letter dated 2nd December 1957)

For the past several weeks I have been studying the Moore/Sylvester-Bradley proposal on parataxa. Though there are certain advantages to it, I am opposed to it for several reasons. This opinion, of course, is mine as an individual and not that of the United States Geological Survey.

2. It is important for zoologists, whether they study Recent or fossil material to think in terms of the " whole animal ". All descriptive material is to a greater or lesser extent incomplete. The fact that in cephalopods, for example, aptychi are less complete, that is have fewer diagnostic characters, than conchs, does not seem to be sufficient reason for discussing the conch as a living animal and the aptychus as an entirely separate object. They are one and the same in being parts of the living animal. Both are incomplete and not " whole animals " ; neither is more of a " whole animal " than the other.

3. It is also important that nomenclature be stable. In my opinion, stability is best achieved by using a single set of simple rules which can be readily interpreted and applied without recourse to another group for rulings. The erection of what is for practical purposes a second system of nomenclature for part of the animal kingdom does not appeal to me as a means of promoting stability. The students of conodonts have difficult problems in the synonomy of generic and specific names, but these are differences in degree only, not in kind, from those which beset the student of any animal group. True stability will be achieved in conodont nomenclature when more is known about the group. Until then, any changes reflecting a clearer understanding of zoologic affinities should be welcomed.

4. Finally, and most important, zoology as such should be left to the zoologists and nomenclature as such should be left to the International

Commission. Moore and Sylvester-Bradley indicate that there is no clear dividing line between taxa and parataxa. If there were, we should all welcome their proposal. The retroactive principal and the concept of a wall between taxa and parataxa certainly trespass on freedom of taxonomic practice.

5. To cite only one specific example, the Commission is asked to rule that a genus *Polygnathus* which was considered by its author to be based ultimately on a natural assemblage, is not based on a natural assemblage. Whether this is a true assemblage or a faecal concentration is beside the point. The point is, that matters of this sort should not be within the province of the International Commission.

CASE No. 3

(*continued from page* 256)

DOCUMENT 3/3

Draft "Règles", Article 13, Section 4(b) : Proposed repeal of Copenhagen Decision 54 (1) (b) (family-group names based on junior homonyms)

(Commission Reference : Z.N.(S.) 1252)

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Statement received on 25th October 1957)

Decision 54 (1) (b) in the *Copenhagen Decisions on Zoological Nomenclature* reads :

> Where the name of the type genus of a taxon belonging to the Family-Group has to be changed because it is found to be a junior hononym, the name of that taxon is to be replaced by a name based upon the changed name of the type genus. (The purpose of this provision is to secure that in the circumstances in question the Family-Group name concerned shall be based upon the oldest available name for a nominal genus which is either objectively, or, if no such name exists, subjectively, identical with the nominal genus, the name of which has been rejected.)

2. There is need for further examination of what nomenclatural acts actually occur when the preceding provision is applied under various circumstances.

3. I have shown in another paper that the type-genus of a taxon of the family-group can only be a nominal, not a taxonomic genus.[1]

4. The name A-IDAE is based upon the nominal genus *A-us*, which is therefore its normal type-genus ; B-IDAE, likewise, is based upon *B-us*. *A-us*

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[1] See Document 11/1.

and *B-us* may be objectively identical taxonomically, but each remains a separate nominal genus, each with its own author and date. In that case A-IDAE and B-IDAE, different nominal families, each with its own author and date, are objectively identical from a taxonomic view-point.

5. Equally, one cannot change the name of a nominal genus. Suppose that *A-us* and *B-us*, two nominal genera, are objective synonyms. Suppose that the valid name for the taxonomic genus which they represent is *B-us*, but that *A-us* has been in use incorrectly. The shift of name of the taxonomic genus from *A-us* to *B-us* is not a change of name of the nominal genus *A-us*. It is selection of the second of two synonyms as the valid name of the taxonomic genus which each represents.

6. With these facts in mind, let us attempt to reword the first sentence of the opening quotation of this paper. It will then read somewhat as follows :—

> If a nominal genus, type of a taxon of the family-group, is found to be a junior homonym and therefore invalid, the nominal taxon of the family-group shall also be deemed invalid. The replacing valid nominal genus automatically becomes type of a new nominal taxon of the family-group. The two nominal taxa of the latter are to be listed as synonyms, just as are the two nominal genera.

7. To illustrate : Suppose that *A-us*, 1796, a genus of birds, is a homonym of *A-us*, 1800, a genus of insects, and that the latter is the type nominal genus of the family A-IDAE, 1815. Suppose that this homonymy was discovered in 1900, and therefore *B-us* was proposed as a new name for the taxonomic genus represented by *A-us*, 1800, *nec A-us*, 1796. The nominal genus *B-us*, 1900, is a valid (although junior) synonym of *A-us*, 1800, which is invalid because of homonymy.

8. The rule that we have been discussing requires that, the above being the case, a new nominal family B-IDAE must be erected to replace the nominal family A-IDAE, now deemed nomenclaturally invalid. The family B-IDAE, 1900, is then a valid (although junior) synonym of A-IDAE, 1815. A-IDAE is not a homonym because no family of birds named A-IDAE has been erected, but it is deemed invalid because it is a potential homonym ; that is, ornithologists must be free to erect a taxon of the family-group with *A-us*, 1796, as its type-genus.

9. This is all sufficiently logical, but I think a contradiction in the *Règles* will have become apparent.

10. Suppose that in 1835 a third genus, *C-us*, within the same taxonomic family of insects, has become type nominal genus of a subfamily C-INAE. This has priority as the name of a taxon of the family-group over B-IDAE, 1900, and the *Règles* state that priority shall obtain among names of the family-group (*Copenhagen Decision on Nomenclature* : 33, Decision 45(1). Therefore by that provision C-IDAE is the valid name of the taxonomic family, while by Decision 54(1)(b) B-IDAE is the valid name.

11. This impasse can be resolved by a new provision to the effect that if the type nominal genera of two nominal taxa of the family-group are objective synonyms, the latter taxon shall be deemed to have the same date of origin, namely the earliest date of either. Thus, in the preceding example, B-IDAE, instead of dating from 1900, would date from 1815, the same as A-IDAE and could then be substituted for A-IDAE.

12. It will occur to those who have read this far that there is still another class of cases that may cause difficulty. As all know, it often happens that when a nominal genus is found to be an invalid homonym, no substitute name is proposed because there already exists a name which is an objective synonym of the invalid nominal genus, or is considered by at least some zoologists to be a subjective synonym of it. If such a nominal genus is the type of a taxon of the family-group, it will be readily appreciated that the situation may be quite different from the case where a name has been expressly proposed as a substitute for the invalid generic homonym.

13. If the invalid generic homonym is replaced by an objective synonym, even though one that has not been expressly proposed as a substitute, the case is not essentially different from às though it had been, and the rule proposed in the preceding paragraph can equally apply. But if it has to be replaced by a subjective synonym, which is probably true in a large majority of such cases, an element of uncertainty enters in, and if such a synonym were allowed to become the type-genus of a new nominal taxon of the family-group, that name would always be unstable, depending upon diverse views of taxonomists, present and future, concerning the synonymy. Even at the outset the newly proposed family-group name would have to be rejected by every taxonomist who refused to accept the generic synonymy. At least this would be true as long as the rule obtains that the new nominal taxon of the family-group has to be based on the nominal genus that replaces the invalid homonym.

14. Under such circumstances it would seem better to let priority govern. Thus, suppose that A-IDAE, 1815, has to be abandoned in 1957 because *A-us* is an invalid homonym ; then, instead of erecting a new nominal family B-IDAE, 1957, based on a subjective synonym of *A-us*, one would search for the oldest family-group name that had been proposed, and this would become the valid name of the taxonomic family formerly called A-IDAE. Only in case no such name existed would a new nominal family have to be erected, not advisably on *B-us*, for the author could and should choose a type nominal genus of which the status was not in doubt. A decided advantage of this method is that it would usually result in the adoption of a name already familiar as that of a subfamily or tribe instead of a wholly new one. If a family were concerned, a familiar subfamily name might replace it. This would leave one taxonomic subfamily without a name, and a known tribal name might be available as a replacement. Then, only at the lowest and least important level, a new nominal tribe might have to be erected.

15. While the methods that I have suggested seem to be those that most closely comply with the intent of the Copenhagen provision, it will be well, now that the subject is open, to consider another approach that would cause less nomenclatorial upheaval. This method is to strictly follow priority in the names of the taxa of the family-group, and *not* to rule that two names of the family-group based on objectively synonymous nominal genera, are to be deemed established at the same date.

16. For example, if *A-us*, 1800, is an invalid homonym, then A-IDAE, 1815, is an invalid family name. Another nominal family must be found or established to replace the latter when the homonymy is discovered, say in 1957. If there is within the taxonomic family another nominal genus or genera which have become each the type of a taxon of the family-group (as tribe or subfamily), then the one first made such a type, say *C-us*, established in 1835 as type of C-INAE, automatically becomes type of C-IDAE, which is then the valid synonym of invalid A-IDAE. This is also the simpler rule to follow because it then makes no difference whether the invalid name *A-us* was replaced by an objective or a subjective synonym.

17. From these considerations I have to propose that one of two courses should be decided upon, and the appropriate amendments, as indicated below, adopted at the London Congress, either course to be preceded by annulment of paragraph 54(1)(b) of the Copenhagen Congress.

Alternative " A "

18. If the first plan is adopted, the following amendments will require to be adopted :

(1) A nominal taxon of the family-group is invalid if its type nominal genus is a junior homonym.

(2) If a nominal taxon of the family-group that is invalid under the preceding provision has a synonym, and if the type nominal genera of the two nominal taxa are objective synonyms, the two nominal taxa shall be deemed to have the same date of origin.

Example: *A-us*, 1800, is a junior homonym, therefore A-IDAE, 1815, is invalid. *B-us*, 1900, is an objective synonym of *A-us*, having in this instance been proposed as a substitute name. Therefore B-IDAE, actually proposed in 1900, is nevertheless deemed to date from 1800.

(3) Rule (2) shall not apply in case that prior to the establishment of the objective junior generic synonym, some author has proposed to replace the nominal taxon of the family-group, on the grounds that it is invalid, with whatever subjective synonym has priority.

Example: *A-us*, 1800, is a junior homonym, therefore A-IDAE, 1815, is invalid. C-INAE, 1830, is the oldest subfamily of the invalidly named taxonomic group A-IDAE. C-IDAE, a subjective synonym of A-IDAE, dating then from 1830, was proposed in 1850 to replace the invalid nominal family A-IDAE (*A-us* and *C-us* are not synonyms, but the families are subjective synonyms). *B-us*, 1900, is an objective synonym of *A-us*, but B-IDAE, proposed in 1900 to replace A-IDAE, 1815, then dates from 1900 not 1815, since the name of the taxonomic family had already been changed.

(4) An invalid nominal taxon of the family-group shall be replaced by its oldest synonym, whether objective or subjective.

Examples: (1) *Objective synonomy*. *A-us*, 1800, is a junior homonym, therefore A-IDAE, 1815, is invalid. *C-us*, 1830, is the type of C-INAE, 1830, but was not used for the family. *B-us*, 1900, is an objective synonym of *A-us*, 1800, in this instance proposed as a substitute name. B-IDAE, actually proposed in 1900, dates for purposes of priority from 1815, since no prior proposal had been made to replace the name A-IDAE. Therefore B-IDAE as the oldest synonym, in this case objective, replaces A-IDAE.

(2) *Subjective synonomy*. *A-us*, 1800, is a junior homonym, therefore A-IDAE, 1815, is invalid. *C-us*, 1830, is made type of subfamily C-INAE, 1830. C-IDAE, 1850, is adopted to replace the invalid A-IDAE. Therefore, although *C-us* is not a synonym of *A-us*, C-IDAE is a subjective synonym of A-IDAE. *B-us*, 1900 is proposed as a substitute name for *A-us*, 1800. But B-IDAE, if proposed in 1900 to replace A-IDAE, dates for priority only from 1900, and C-IDAE, 1850, remains the valid name to replace A-IDAE.

(5) Rules (2) to (4) above shall apply to invalid nominal taxa of the family-group if replacement has to be made subsequent to 1958. They shall not be applied in such manner as to disqualify a nominal taxon of the family-group that has been accepted prior to 1958 as the valid synonym of an invalid name.

Alternative " B "

19. By the second and simpler method, the following amendments will need to be adopted :—

(1) Same as under Alternative " A ".

(2) Same as (4) in Alternative " A ".

(3) The preceding rule (2) shall apply to invalid nominal taxa of the family-group if replacement has to be made subsequent to 1958. It shall not be applied in such manner as to disqualify a nominal taxon of the family-group that has been accepted prior to 1958 as the valid synonym of an invalid name.

20. Whichever set of rules is preferred would replace Article 13, Section 4(b)[2] of the proposed Draft of the *Règles*.

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[2] See Vol. **14** : 98.

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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* Professor Esaki died on 14th December 1957, while the present Part was passing through the Press.

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CASE No. 16

DRAFT " RÈGLES ", ARTICLE 6, SECTION 6(b) : PROPOSALS RELATING TO THE FORM OF GENERIC NAMES INTENDED FOR PALAEONTOLOGY

(Commission Reference : Z.N.(S.) 1293)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 57)

DOCUMENT 16/1

Proposed deletion of Article 6, Section 6(b), of the Draft " Règles "

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Division, Washington, D.C., U.S.A.*)

(Enclosure to a letter dated 29th November 1957)

It is proposed that part of (b) of Section 6 be deleted from the Code. I believe that the provisions of the Code should be general in nature, and not attempt to cover particular situations in individual groups. My proposal is based on this principle, and not on the merits of the problem itself; those could be dealt with by the Commission under its Plenary Powers.

DOCUMENT 16/2

The Form of Generic Names intended for Palaeontology : A word of caution on Dr. Sabrosky's proposal

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 31st December 1957)

In his note dated 29th November 1957, Dr. Sabrosky has suggested the deletion from the *Règles* of the provision which at present stands as Article 6, Section 6(b) in the Draft (1957, *Bull. zool. Nomencl.* **14** : 57) relating to the forms of generic names intended for palaeontology. In submitting this proposal Dr. Sabrosky explains that he does so because in his opinion the *Règles* should not attempt to cover particular situations in individual groups, adding that if difficulties were to arise in such cases they could be dealt with by the Commission under its Plenary Powers.

2. In general I would agree with Dr. Sabrosky that care should be taken to include in the *Règles* only provisions of a general character. It seems to me, therefore, that what is required in the present case is advice from palaeontologists as to whether the problem dealt with in the proposal, sought to be deleted by Dr. Sabrosky, is in their opinion one of sufficiently general character to justify inclusion in the *Règles*.

3. Looking at this matter purely from the point of view of the procedure of the International Commission, I must say that, if palaeontologists were to be of the opinion that this is a useful provision and that its omission would be likely to give rise either to lack of uniformity or to the submission to the Commission of any considerable number of applications, I would favour the retention of the provision drafted by Professor Chester Bradley. It is important that the *Règles* should not deal with matters which could better be dealt with on an individual basis by the Commission, but it is important also to avoid falling into the opposite error by omitting provisions from the *Règles*, where to do so would invite the submission to the Commission of possibly considerable numbers of applications which would be unnecessary if the provision in question had been inserted in the *Règles*.

CASE No. 17

ARTICLE 22, RECOMMENDATION 10(β) OF THE DRAFT RÈGLES CITATION OF DATES IN BIBLIOGRAPHICAL REFERENCES

(Commission Reference : Z.N.(S.) 1294)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 166)

DOCUMENT 17/1

Proposed deletion of Article 22, Recommendation 10(β) relating to citation of dates in a particular situation

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Division, Washington, D.C., U.S.A.*)

(Enclosure to letter dated 29th November 1957)

It is proposed that item (ii) under *Recommandation* 7 be deleted. That is a pedantic provision that would be ignored by most zoologists. The difference between (1958) and [1958] would not be apparent to most, and perhaps is a fine distinction that need not be made anyway.

DOCUMENT 17/2

Proposed retention of the Paris Congress decision at present embodied in " Recommandation " 10(β) of Article 22 of the " Règles "

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 31st December 1957)

Dr. Sabrosky (in Document 17/1) suggests that the London Congress should reverse the decision taken by the Paris Congress in 1948 that zoologists should be recommended to place the date of a given name in round brackets (parenthesis) when that date is obtainable only from indirect evidence provided by the work itself. The purpose of this decision by the Paris Congress was to provide a ready means of distinguishing a date ascertainable only in the above manner from a date which can only be determined by reference to some external source. As regards the latter the Congress recommended that the date should be enclosed in square brackets (brackets). The provision, which Dr. Sabrosky suggests should be omitted, corresponds with the practice of the British Museum (Natural History) and other leading bibliographical authorities and its use often proves of material advantage to later authors when considering the date applied to a name by some earlier author.

2. I would agree with Dr. Sabrosky that this is a *Recommandation* which some authors and editors might perhaps ignore. This does not seem to me, however, to be a good reason for not including in the *Règles* a provision which has proved intrinsically useful and which, moreover, corresponds with the best bibliographical practice. A provision of this kind would not be suitable for inclusion in the *Règles* as a mandatory provision, but this is not the proposal in Professor Bradley's draft where it is recommended that this matter should be dealt with in a non-mandatory *Recommandation* non-compliance with which would not involve an actual breach of the *Règles*.

3. For these reasons I recommend that the Paris Congress decision in this matter be upheld and that, in consequence, the provision discussed above be retained in the *Règles*.

CASE No. 18

DRAFT " RÈGLES ", ARTICLE 24, SECTION 15(e) : REPLACEMENT OF JUNIOR HOMONYMS POSSESSING ONLY SUBJECTIVE SYNONYMS

(Commission Reference : Z.N.(S.) 1295)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 185–186)

DOCUMENT 18/1

Proposed Redraft of Section 15(e) of Article 24 of Draft " Règles "

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Division, Washington, D.C., U.S.A.*)

(Enclosure to a letter dated 29th November 1957)

It is proposed that part (e) as it stands be deleted and be replaced with the following, under the same letter and title : " If the junior homonym to be replaced has no objective synonym, but already has one or more subjective synonyms, the rejected homonym is to be replaced by its earliest established available subjective synonym for so long as that name remains subjectively identified with it. If that species ceases to be regarded as subjectively synonymous, the next earliest shall be used. If no other subjective synonyms are known and available, the junior homonym shall be renamed ".

DOCUMENT 18/2

Points for consideration on Dr. Sabrosky's proposal relating to the replacement of junior homonyms possessing only subjective synonyms

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

The question of the procedure to be followed when a name is found to be invalid as a junior homonym of some older available name was discussed at considerable length at the Paris Congress in 1948. It was then generally agreed that, other things being equal, the logical course would be to replace with a new and available name any name found to be invalid as a junior homonym of some other name, for the adoption of this course would make it possible in discussing subjective synonymy to refer to the taxon concerned by a name which was nomenclatorially available. On the other hand, the view was expressed that in a very large number of cases the circumstances were such that no specialist in the group concerned would be likely to take the view that the taxon to which had been given the name to be rejected as a junior homonym was without a name which, on the subjective plane, was clearly applicable to it. In these circumstances, it was argued that the automatic replacement by new names of names rejected as junior homonyms would lead to the introduction into the literature of many names which in fact would never be used. This latter view won the day, and it was for this reason that the Paris Congress agreed to include in the *Règles* a provision under which it would be made clear that an invalid homonym could be replaced by a new name or be replaced by the existing subjective synonym of later date, whichever was considered the more appropriate.

2. Dr. Sabrosky's proposal has the effect of shifting the emphasis in favour of the second of these courses, that is, against the giving of a new name and in favour of using a subjective synonym. It seems to me that, subject to drafting points noted below, Dr. Sabrosky's proposal has the advantage of greater brevity, while securing substantially the same ends as those agreed upon in Paris. The drafting points which I should like to suggest for consideration are the following :—

(a) *First phrase of first sentence* : It would, I think, be well to insert the words " and nomenclatorially available " between the words " objective " and " synonyms " because a junior homonym might have an objective synonym which, for some reason, was itself invalid.

(b) *First sentence, second portion*: For reasons similar to those explained in (a) above, it would, I think, be prudent to insert the words " and nomenclatorially available " between the words " subjective " and " synonyms ".

(c) *First sentence, main phrase*: There may be situations, for example in revisional work, where it is desirable, in the view of the specialist concerned, that an invalid homonym should be replaced, even though there may exist a name which is, or is claimed to be, a subjective synonym. This possibility was provided for by the Paris decision but is entirely ruled out by the actual words in Dr. Sabrosky's draft. I suggest that if Dr. Sabrosky's draft is to be adopted, it should be qualified by the insertion of the words " in general " between the word " is " and the words " to be replaced ". This small addition, while meeting the point referred to above, maintains, as it seems to me, the general purpose of Dr. Sabrosky's proposal.

(d) *Last sentence, at end*: In view of the confusion which has occasionally arisen in the literature about the attribution and dating of substitute names, it would, I think, be helpful if at the end of this sentence there were to be added the words " a substitute name so published to be attributed to the author by whom it was published and to the date on which it was published ", unless it is considered that words to the same effect can be more conveniently inserted in some other part of the *Règles*.

CASE No. 19

Draft " Règles ", Article 26 : banning of the use of intemperate language in discussions on zoological nomenclature

(Commission Reference : Z.N.(S.) 1296)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 190)

DOCUMENT 19/1

Proposed deletion of Article 26 in the Draft " Règles "

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Division, Washington, D.C., U.S.A.*)

It is proposed that this entire Article be deleted from the Code. It is an insult to the great bulk of zoologists, and it is wishful thinking to believe that it will have any effect whatsoever on those who do use it, except perhaps to intensify their intemperacy. Language is a matter of good taste, but this is subjective. What one man considers intemperate will seem merely picturesque or forceful to another. The interpretation may vary according to whether or not one agrees with the speaker or writer. Actually, really intemperate language will in these times rarely, if ever, get past an editor.

DOCUMENT 19/2

Proposed retention of the provision banning the use of intemperate language in discussions on zoological nomenclature

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 10th December 1957)

In his note ,dated the 29th November 1957 (Document 19/1), Dr. Curtis W. Sabrosky recommends the deletion from the *Règles* of the provision banning the use of intemperate language in discussions on zoological nomenclature.

2. I recommend that Dr. Sabrosky's proposal be rejected. The provision to which he takes exception was unanimously adopted by the International Commission on Zoological Nomenclature at its Session held in Monaco in March 1913, and was approved by the Ninth International Congress of Zoology meeting at that time. This resolution was subsequently embodied in *Declaration* 4 (1943, *Ops. Decls. int. Comm. zool. Nomencl.* **1**(A) : 23–30), which contains in full the discussion deprecating the use of intemperate language included by my predecessor the late Dr. C. W. Stiles (*Washington, D.C.*) in the Report which he prepared for, and which was adopted by, the International Commission at Monaco. This *Declaration* was considered again by the International Commission at Paris in 1948, and on its recommendation was incorporated into the *Règles* by the Thirteenth International Congress of Zoology then meeting in that city (1950, *Bull. zool. Nomencl.* **4** : 167). The provision in question has thus been part of the body of international rules relating to zoological nomenclature for over forty years, and, for this reason alone, it would seem to me a mistake to revoke this provision, unless there were any strong grounds for so doing.

3. It will, I think, be generally agreed that during the last half century the standard of politeness in papers on zoological nomenclature has greatly improved, personal attacks and the use of offensive epithets in zoological papers having been of much rarer occurrence than in earlier periods. No doubt this change is due in part to the general improvement in manners during the period, but it may, I think, be claimed that the resolution, adopted by the Commission and approved by the Congress in 1913, deprecating the use of offensive language contributed to the improvement which has taken place. It would appear to me to be a retrograde step at this stage to cancel the existing provision on this subject.

4. Dr. Sabrosky is no doubt right when he says that " really intemperate language will in these times rarely, if ever, get past an editor ". It is also true, however, that the existence of the present provision has often proved of assistance to editors when seeking to delete offensive passages from papers submitted for publication. I might perhaps add that, as Editor of the *Bulletin of Zoological Nomenclature*, I have on more than one occasion found the existence of this provision of value when editing papers for publication.

CASE No. 20

DRAFT " RÈGLES ", ARTICLE 28, SECTION 9(a) AND APPENDIX : GRAMMATICAL FORMATION OF FAMILY-GROUP NAMES

(Commission Reference : Z.N.(S.) 1266)

(For the relevant provisions in the Draft of the Revised *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 204, 233–237.)

DOCUMENT 20/1

The grammatical formation of names for taxa belonging to the family-group category

By **J. CHESTER BRADLEY**

(*Cornell University, Ithaca, N.Y., U.S.A.*)

(Enclosure to a letter dated 14th November 1957)

Formerly the rule that the name of a taxon belonging to the family-group category was to be formed by adding the appropriate suffix to the stem of the name of its type-genus gave rise to a good deal of uncertainty, because zoologists untrained in the classics did not always recognize the stem of a classical word.

2. When at Copenhagen (1953, *Copenhagen Decisions zool. Nomencl.* : 34, Decision 50(a)) the rule was changed, so far as classical words are concerned, to require that the genitive case-ending of the word concerned be replaced by the appropriate family-group suffix it was believed that the matter had been so simplified as to become routine for any zoologist to use. Since the genitive is given in classical lexicons and the genitive case-ending in grammars, any one could easily form the family-group name, without any knowledge of Greek or Latin.

3. The Consulting Classical Adviser to the Commission, Professor L. W. Grensted, has pointed out to me that the rule can only apply if the name, or the last part of the name, is a Greek or Latin word. The wording " of Greek or Latin origin ", he points out, can lead to misunderstanding and confusion.

4. Any word that is a generic name is by that fact a Latin or Neo-latin word. If it has been transliterated from the Greek with the Greek termination, that is to say, employing the Greek nominative case-ending, then and then only will it retain its original Greek stem. If it has been given a different Latin nominative case-ending, then it will acquire both the stem and the gender appropriate in Latin to the new ending. " The Greek stem " writes Professor Grensted " only survives when the Greek word survives entire and complete. Then in classical Latin and in zoological usage the Greek stem is retained too, but not otherwise ". " The name, once coined, becomes Latin, and the stem required is the Latin stem ".

5. For example, take generic names ending in the Greek word κέρας (keras) a horn, or ending in a Latinized derivative of that word. The Greek κέρας has an increasing genitive κέρατος (keratos) the termination (genitive case-ending) of which is -os, and when this is eliminated there remains the combining form κέρατ- to which the family suffix -idae must be added to form the family name. The Greek K (kappa) is transliterated into Latin as c. Thus a family name based on the generic name *Calliceras* would be CALLICERATIDAE. If, however, in forming a Neo-Latin generic name from κέρας a new nominative case-ending has replaced the -ας, the original Greek stem has been lost, and the word must be declined as any other Latin word. Thus in *Trichocera* the nominative case-ending -a has replaced the Greek -ας, and the word both as to stem and gender must be treated as a Latin noun of the First Declension and as of the feminine gender. The genitive singular is *Trichocerae*, the family name TRICHOCERIDAE. In *Heterocerus* the nominative case-ending " *-us* " is employed, and the word is a noun of the Latin Second Declension and is masculine in gender, with its family name HETEROCERIDAE.

6. In order that this matter may be corrected, and clarified, I would suggest that in the Revised *Règles* an amendment should be made in the portion embodying the opening phrase of Copenhagen Decision 50(1)(a) (1953, *Copenhagen Decisions zool. Nomencl.* : 34) and an " Explanation " added. In addition, a further short " Explanation " is suggested, together with a proposal for the amendment of one phrase wherever it occurs in the Draft *Règles*. The proposals so submitted are the following :—

(1) Draft " Règles ", Article 28, Section 9(a), opening phrase[1] :—

The words " a word of Greek or Latin origin " to be replaced by the words " a Greek or Latin word ".

(2) " Explanation " proposed to be inserted :—

Explanation : If a Greek word is retained in a newly coined Neo-Latin generic name without change of nominative case-ending it retains its Greek stem and genitive. If a Latin nominative case-ending is employed, both the stem and the gender become whatever is appropriate in Latin to the new ending. *Examples* : The following generic names all derive from the Greek word κέρας (keras), transliterated into Latin " *ceras* ") a horn ; (a) *Calliceras*, using the Greek nominative, retains the Greek stem and gender, and a family name based on it would be CALLICERATIDAE (genitive *Calliceratos*, stem *Callicerat-*) ; (b) In *Trichocera* the Latin nominative ending " -a ", has been substituted and this makes it a feminine word of the First Declension, with genitive *trichocerae*, family name TRICHOCERIDAE ; (c) In *Heterocerus* the nominative case-ending *-us*, has been substituted and this makes it a masculine noun of the Latin Second Declension, genitive *heteroceri*, family name HETEROCERIDAE.

(3) Supplementary " Explanation " proposed to be added :—

The table of Latin nouns set out in the Appendix to Article 28[2] may be consulted in such cases to assist in determining the proper form of the genitive of a noun and its gender.

(4) Proposed substitution of the expression " nominative case-ending ' for the expression " nominative suffix " :

The expression " nominative suffix " used in the Draft *Règles* means " nominative case-ending ", but the latter expression is more easily understood and should, it is recommended, be substituted in the Draft *Règles* for the expression " nominative suffix ", wherever the latter expression occurs.

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[1] The prase here referred to appears in lines 10 and 11 on page 204 of the Draft *Règles* (1957, *Bull. zool. Nomencl.* 14 : 204).

[2] For the Appendix here referred to see pages 233–237 of the Draft *Règles* (1957, *Bull. zool. Nomencl.* 14 : 233–237).

CASE No. 21

DRAFT " RÈGLES ", ARTICLE 28, SECTION II : GENDER OF GENERIC NAMES

(Commission Reference : Z.N.(S.) 996)

(For the relevant provisions in the Draft of the Revised *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 208 *et seq.*)

DOCUMENT 21/1

Report on the Rules for the determination of the gender attributable to generic names of various classes adopted by the Copenhagen Congress of 1953 in its Decision 84

By **L. W. GRENSTED**

(*Consulting Classical Adviser to the International Commission on Zoological Nomenclature*)

(Report dated 14th January 1957)

In accordance with the request contained in your letter of 30th November 1956 I have examined the rules for determining the gender attributable to generic names laid down by the Fourteenth International Congress of Zoology, Copenhagen, 1953 by its Decision 84 (1953, *Copenhagen Decisions zool. Nomencl.*: 49–51) and submit the following Report on the matters on the nature of the amendments to those Rules which, in my opinion, are desirable under the review called for under Decision 85 (*loc. cit.* : 51) of the above Congress.

2. Section 84(3) of the Copenhagen Decisions reads as follows :—

> Where a Greek or Latin word has, on being used as a generic name, been modified by the addition of a nominative suffix, the gender becomes that of the Suffix (thus nouns ending in "-stomus" are to be treated as masculine, although those ending in "-stoma" are neuter).

This recommendation has resulted in difficulties of interpretation when read in the light of Section 84(7)(b)(1) where names ending in "*-gnathus*" are determined as feminine, as being "obviously derived from Greek words, which are given in the standard Greek lexicon as being feminine in gender", and of Section 84(7)(c)(1) where names ending in "*-cheilus*", "*-rhamphus*", "*-rhynchus*", and "*-stathus*" are determined as neuter, as being "obviously derived from Greek words of neuter gender, by reason of having the termination "*-os*" ".

3. Two preliminary points arise in connection with these two clauses (as well as certain others) of Section 84(7) :—

(i) The phrase "obviously derived from" is far too loose, and might give rise to many problems of interpretation. Thus, to take the example given in Section 84(3) "*-stomus*" is "obviously derived from" the neuter noun *stoma*, but is nevertheless determined as masculine, by reason of the nominative suffix "*-us*".

(ii) The Greek termination of "*-os*" is masculine in the vast majority of cases and cannot be cited as a reason for a neuter gender. Forms such as *gnathos*, *cheilos*, etc. are very few, constituting a small and special class.

There is clear need for careful re-drafting of Section 84(7) with these points in view.

4. But the most serious difficulty arises in connection with the decision that forms in *-gnathus* should be feminine, and forms in *-cheilus* (or *-chilus*), *-rhamphus* etc. should be neuter. For by Section 84(3) it appears that, for example, *-cheilus* is the Greek *-cheilos* transformed by the addition of the Latin nominative suffix " *-us* ", which is masculine, exactly as *-stoma* (the example cited) is the Greek *-stoma* transformed by the addition of the Latin nominative suffix "*-us*". It is agreed that in this latter case the gender is masculine. There is every reason to apply the same reasoning to the cases cited above of forms in *-gnathus*, *-cheilus* etc. A ruling has in fact been recently given to the effect that the generic name *Oxycheilus* (or *Oxychilus*) is, as it has always been regarded in taxonomic usage, masculine, a decision reversing Section 84(7)(c)(1), but agreeing with Section 84(3).

5. Against this it might be argued that *-cheilus* is simply a transliteration of the Greek *-cheilos* (χεῖλος), and that a form such as *Oxycheilus* therefore ends in a noun given in the Standard Greek Lexicon as neuter. This is indeed the only ground for attaching the neuter gender to this and similar generic names. There are good reasons for rejecting this argument, as follows :—

(1) *-cheilus* (and similar forms) are not transliterations but Latinisations. The transliteration of χεῖλος would be *cheilos* and there are cases in scientific Latin where this transliteration of the Greek "*-os*" occurs, as in *Nymphalis polychloros*, and, in principle in such generic names as *Ennomos*. But a Latinised *Oxycheilus* would certainly, being strictly an adjectival form, have been treated as masculine.

(2) The "*-us*" in this case must certainly be regarded rather as a masculine "nominative suffix" than as a transliteration. In other words names such as *Oxycheilus* do not end in a noun given in the standard lexicon as neuter, but in a modified form, masculine by its termination.

(3) A more obscure point, but in fact an equally decisive one, is that names of this type, if put back into their Greek form, would be two-termination adjectives. Thus *Oxycheilus* would be ὀξύχειλος (sharp-lipped) and the "*-os*" would not be the termination of the noun χεῖλος but the ordinary masculine "*-os*" of the common adjectival form. Such adjectives are abundant in Greek, and there would be no doubt whatever that a name taken over from them, either in Greek or in a Latinised form, would be masculine.

It is in fact not strictly true that *Oxycheilus* ends in the Greek noun "*-cheilus*". It is, properly speaking, an adjectival form, with stem "*Oxcheil-*" and the Latin masculine termination "*-us*".

(4) To retain the masculine gender for these nouns would be, largely or wholly, in line with taxonomic tradition.

6. It is clear that this discussion renders a revision of the language of Section 84 very desirable, on lines sufficiently indicated above.

CASE No. 22

DRAFT "RÈGLES", ARTICLE 24, SECTION 11(c): PROVISION FOR CHALLENGING THE REJECTION OF SECONDARY HOMONYMS

(Commission Reference : Z.N.(S.) 1275)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* **14** : 180)

DOCUMENT 22/1

Proposal to eliminate the provision for Challenge of the Rejection of Secondary Homonyms

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Division, Entomology Research Division, Washington, D.C., U.S.A.*)

(Enclosure to a letter dated 17th October 1957)

Article 24, Section 11(c) of the Draft of the new Code provides, in line with Decision 162 of the Copenhagen Congress (1953, *Copenhagen Decisions zool. Nomencl.* : 82–83), a procedure for challenging the rejection of a secondary homonym. The procedure seems unreasonably drastic and open to abuse, and hence undesirable, for the following reasons :—

(a) There is provision for challenge, but none for consideration of the merits of a challenge. Thus one or a few individuals—possibly a small minority in a given field of work—would be given a veto power over the status of a replacement name, merely by entering a protest.

(b) Difference in zoological views may be wide, but sincere, and reasonable allowance must be made. It was not the intent of the Copenhagen Decision, from the discussion at the Colloquium, to cover all ordinary cases, but to provide an escape mechanism against flagrant and meddlesome examples of unjustified synonymy leading to homonyms and consequent replacement names. Yet there is evidence that, as presently worded, the provision will be invoked against ordinary changes made in good faith by reputable zoologists.

(c) There is no stated time limit on the replacement names which may be challenged under the provision. In a case known to me, a challenge was drafted against a replacement name over thirty years old, a name proposed in good faith and accepted and used by a number of authors.

2. Particularly flagrant examples could always be handled under the Plenary Powers, without writing into the Code a loose provision that would breed disputes and litigation.

3. It is therefore proposed that paragraph c of Section 11 of Article 24 of the Draft Code be revoked and stricken from the Code.

DOCUMENT 22/2

Support for Dr. Sabrosky's proposal for the repeal of the provision agreed upon by the Copenhagen Congress for the application of the "Notification and Challenge" procedure in relation to secondary homonyms

By **FRANCIS HEMMING**, C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 10th November 1957)

I desire strongly to support Dr. Sabrosky's proposal for the deletion from the Draft *Règles* of the provision embodying the decision of the Copenhagen Congress to provide what it was hoped would be a substantially automatic procedure for challenging the rejection of names as secondary homonyms.

2. Everyone will agree that there should be appropriate means for securing the rejection of names published as substitute names for names rejected as secondary homonyms in cases where the circumstances leading to the condition of homonymy are such as would never have arisen if the author publishing the replacement names had acted with greater discretion. The opportunities, however, for the unnecessary rejection of names as secondary homonyms are not great, and I agree with Dr. Sabrosky in considering that the best way of dealing with such cases, if detected, would be by submission of the relevant particulars to the International Commission, with a request for the suppression under the Plenary Powers of any replacement names unnecessarily brought into existence in this way. In this connection, however, it will be necessary to make sure that the final text in the *Règles* regarding the use of the Plenary Powers is drawn sufficiently widely to prevent any question being raised as to the appropriateness of their use for the foregoing purpose.

3. I am reinforced in my view that the proposal submitted in this matter by Dr. Sabrosky deserves the fullest support by my conviction that the "Notification and Challenge" procedure is inherently dangerous, and, as Dr. Sabrosky observes in the present case, is likely to "breed disputes and litigation", that is, that instead of promoting stability in nomenclature, as it was hoped at Copenhagen that it would do, this procedure if applied, would, in fact, give rise to instability and confusion. My reason for regarding this mechanism as misconceived is twofold: (1) Experience in the day-to-day work of the Commission has shown that even the publication of a full statement of a case in the *Bulletin of Zoological Nomenclature*, coupled with the issue of Public Notices, cannot be relied upon in every case to provide an adequate assurance that the proposals submitted command the support of the general body of specialists in the group concerned. In consequence in cases of this kind I have felt it necessary, as Secretary to the Commission, myself to initiate consultations with specialists before submitting the application concerned to the Commission for vote, in order thereby to satisfy myself that the lack of comments received was due to inertia or preoccupation with other duties on the part of specialists in the group in question and was not attributable to hostility to the action recommended by the applicant. (2) If even the full-dress procedure of publication, followed by the issue of Public Notices, cannot be relied upon in every case to provide a reasonable assurance that *fiat* action is desirable and generally supported, it must be obvious that the supply of the meagre particulars which alone are called for under the " Notification and Challenge " procedure would be totally insufficient to provide adequate guarantees that its application would produce satisfactory results. Indeed, as Dr. Sabrosky justly remarks in the present instance, the use of this procedure might easily have the effect of putting minorities in a position of imposing their will upon their more numerous—and possibly more responsible, though less vocal—colleagues. While strongly endorsing Dr. Sabrosky's plea in the present case, I would add that from my own experience I am convinced that the " Notification and Challenge " procedure suffers from such serious inherent defects that it cannot be relied to promote stability or uniformity, being more likely, on the contrary, to lead to uncertainty and confusion.

CASE No. 23

DRAFT "RÈGLES", ARTICLE 6, SECTION 1(d); ARTICLE 10, SECTION 2(a); ARTICLE 23, SECTION 1(a)(i): BANNING OF NAMES CALCULATED TO GIVE POLITICAL, RELIGIOUS OR PERSONAL OFFENCE

(Commission Reference: Z.N.(S.) 1297)

(For the relevant provisions in the Draft see 1957, *Bull. zool. Nomencl.* **14**: 49, 85, 167)

DOCUMENT 23/1

Deletion from the "Règles" of the provisions relating to the rejection of names calculated to give political, religious or personal offence

By **CURTIS W. SABROSKY**

U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Division, Washington, D.C., U.S.A.)

(Enclosure to a letter dated 29th November 1957)

It is proposed that the three provisions in the Draft *Règles* (Article 6, Section 1(d); Article 10, Section 2(a); Article 23, Section 1(a)(i)) dealing with offensive names be deleted from the Code, or at most reduced to *Recommendations*.

Provisions dealing with the offensiveness of names call forth the same comments made in an earlier proposal relating to "The Use of Intemperate

Language " (Article 26)[1]. Furthermore, there will be grave difficulties in framing fair and effective rules. Intent would be very difficult to prove, and the expression " in any language " covers a lot of ground. Good taste and editorial taste will take care of the really serious cases. The rare examples that really cry for relief can always be handled by the Commission under its Plenary Powers.

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[1] For Dr. Sabrosky's proposal for the deletion from the *Règles* of the provision banning the use of intemperate language in discussions on zoological nomenclature see Document 19/1 (: 328)

DOCUMENT 23/2

Support for the retention in the " Règles " of the provisions banning the use of offensive words as zoological names

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 10th December 1957)

In his note, dated the 29th November 1957 (Document 23/1), Dr. Curtis W. Sabrosky recommends the deletion from the *Règles* of the provisions banning the use of offensive words as zoological names. I recommend the retention of this provision and, therefore, the rejection of Dr. Sabrosky's proposal.

2. The provisions which Dr. Sabrosky seeks to delete from the *Règles* were inserted on the recommendation of the Commission by the Thirteenth International Congress of Zoology, Paris, 1948 (1950, *Bull. zool. Nomencl.* **4** : 193—194). Dr. Sabrosky explains that his objections to the existing provisions are similar in some respects to his objection to the proposal banning the use of intemperate language in discussions on zoological nomenclature.

3. So far the provisions now in question have been used by the International Commission on one occasion only. This arose on applications submitted by Dr. Joshua L. Baily, Jr. (*San Diego, California, U.S.A.*) and Dr. Myra Keen (*Stanford University, Stanford, California, U.S.A.*), each of whom asked for the rejection of a generic name in the Class Gastropoda which consisted of a word which in the Lapp language, signifies the Christian Deity and is so used in translations of the Bible in the Lapp and Finnish languages. In this case the Commission came to the conclusion that the objections advanced were well founded and, accordingly, directed that the name in question (*Jumala* Friele) be suppressed. The decision so taken has been embodied in *Opinion* 469 (1957, *Ops. Decls. int. Comm. zool. Nomencl.* **16** : 97–128).

4. In the case referred to above the provisions adopted by the Paris Congress proved of practical value. If no such provisions had existed, it would have been necessary for the Commission, if asked to deal with such a case, to have proceeded under its Plenary Powers, a procedure which with its two-thirds majority rule was considered in Paris to be inappropriate for dealing with cases of alleged blasphemy and the like. Quite apart from this consideration, a decision to delete from the *Règles* a provision of ten years' standing banning the use of offensive words as zoological names would, if taken by the London Congress in 1958, be calculated to give the impression that in this matter that Congress took a more lax view than that held by its predecessors. (Dr. Sabrosky's alternative suggestion that the provisions decided upon by the Paris Congress, if (contrary to his advice) these were to be retained at all, should be downgraded to the status of non-mandatory *Recommandations* does not meet the issue involved which was not merely to deprecate the use of offensive words as zoological names but to provide a ready means for invalidating such names.)

DOCUMENT 1/49

(*continued from page* 314)

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

(Commission Reference : Z.N.(S.) 1056)

By **C. W. WRIGHT**

(*London*)

(Enclosure to a letter received 7th December 1957)

I write as a specialist in Mesozoic Ammonoidea, Echinoidea, Asteroidea and Brachyura.

2. Professor Moore kindly sent me advance copies of his applications. I have carefully considered these proposals and have concluded that they are unnecessary and open to objection.

3. The great majority of palaeontological taxa are, as Moore and Sylvester-Bradley admit, founded on more or less incomplete remains of organisms. Many of these fragmentary remains consist of discrete parts, for example the guards of belemnites, the marginalia of asteroids, the cephalothoraces of brachyurous crustacea. In the cases mentioned sufficient deductions can be made from the available evidence to allow a reasonable and useful classification to be devised. This was not always the case in these groups. Knowledge has advanced.

4. At the present day there remain a few well-known types of discrete parts of organisms which are often found isolated from other parts and which are not identifiable as to " whole-animal genera and species " in the conventional sense. The problem of these groups does not differ in principle but only in degree from that of very many, perhaps a majority, of other sorts of fossil remains. I would emphasise moreover that not all the groups mentioned by Moore and Sylvester-Bradley (paragraph 3) " constitute a special category of zoological entities ". In particular marginalia of asteroids can normally be classified in the same system as whole animals.

5. It is for those who wish to alter fundamentally the basic principle of zoological nomenclature, one name for one kind of animal, to demonstrate the necessity of change and the harmlessness of their proposals. Moore and Sylvester-Bradley have in my view, done neither.

6. Since there is no group of cases that differs in principle from those of other fossil remains I shall consider a few particular examples.

Aptychi

These are, at any rate in the main, the opercula of ammonites. As Moore and Sylvester-Bradley explain, the danger of instability and confusion in the nomenclature of ammonites arises because there exists a, limited, number of technically valid and available aptychus names that are prior to the names of the ammonite shell species or genera to which they are thought to belong. However, few if any aptychi can be assigned without doubt to species of ammonites founded on shells. Consequently it is impossible to determine to which taxonomic species any aptychus belongs and there is therefore a good case for regarding all aptychus names as *nomina dubia* and referring them to the Commission for decision. Much the same situation applies in the case of generic aptychus names since most aptychus genera can only be assigned, at the best, to groups of shell genera. In any case the difficulties can readily be resolved in a way that entirely accords with the practice of most authors for the past century on the lines recommended in his application by Dr. Arkell, by suppressing all aptychus names for purposes of zoological nomenclature. There would be no practical loss to palaeontologist or stratigrapher. The former would continue to use as technical terms or vernacular names the " genera " and " species " in the literature and attribute to species or genera of ammonites in so far as knowledge allowed. The latter could similarly use such terms in the titles of or information about the rare beds which are solely characterised by aptychi. The potential confusion in the nomenclature of ammonites must obviously be cleared up but confusion will only arise in practice if an author deliberately tries to create it.

Conodonts

These are discrete parts of animals of uncertain affinities. Each animal had several pairs of " teeth " of a variety of shapes. Every shape has been given a separate specific name and like shapes have been grouped into " genera ". Each individual animal included parts assigned to several " genera " and " species " (*cf.* Moore's digest, paragraph 4(b[1])). There is little reason to doubt that eventually it will be possible to assign to certain whole-animal species most of the " form " genera and species that now exist, but the process will take some years. During the transitional period, which has already begun, there will be very serious problems of nomenclature on which the Commission will have to advise, but I find it hard to believe that a system that perpetuates two independent nomenclatures could be of permanent value to palaeontologist or stratigrapher.

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[1] Reproduced as Document 1/15

Holothuroids

Embedded in the skin of sea cucumbers are calcareous spicules or plates, of several sorts in each species. A few American authors, quoted by Moore and Sylvester-Bradley, have insisted that a separate classification, up to family level, should be erected for each main type of spicule, since the spicules are usually found fossil only in isolation from each other. Thus they propose a family for " wheel " spicules and a family for " hook " spicules, although they were well aware that wheels and hooks occur together in living forms. There was recently described (Hodson *et al.* 1956, *Geol. Mag.*, **93** : 336—344 an important fossil association of wheels and hooks, obviously belonging to one individual. Yet the wheels were referred to a new species of one genus and the hooks to a new species of another. Such a procedure cannot advance either palaeontology or stratigraphy and its justification appears to lie only in the easy cataloguing of isolated spicules.

7. In palaeontology one is often forced to resort to an open nomenclature, for example " A-idae, gen. nov. ? ", " *X-us* sp." or " Indeterminate crinoid brachials ". Such nomenclature determines to the limits of knowledge. So far as zoology goes it is all that is justified or required. As knowledge grows, it is often possible to improve on such open classification. The existence of a legitimated system of parataxonomy would provide one more temptation not to seek enlightenment. For the stratigrapher any accepted system of nomenclature is adequate. If there is need for stratigraphical purposes of something more detailed or precise than the type of open nomenclature referred to above, there are many simple ways in regular use. For example for many years the varying shapes of calyces of the small crinoid *Bourgueticrinus*, which are of great stratigraphical value in the English Upper Chalk, have been referred to as " Brydone's Form. 2 ", etc., with no possibility of confusion and no prejudging of the hitherto unsolved question of the zoological importance of the various shape variations.

8. In my view the Moore/Sylvester-Bradley proposals tend to erect into a principle a current and perhaps ephemeral lack of knowledge (e.g., in the case of conodonts), or the accident that some hard parts of some animals are of different chemical composition than other hard parts and are therefore differentially preserved (e.g., in the case of aptychi). Moore and Sylvester-Bradley are in fact proposing to use a sledgehammer to crack a nut and indeed to contravene William of Occam's principle, so valuable in all the realms of human thought, that entities should not be multiplied unnecessarily.

9. There are, however, certain more positive objections to these proposals. It is notorious that even now, after so many years existence of the current system, only a small proportion of palaeontologists are thoroughly familiar with the *Règles* and try seriously to obey them. Indeed in France the regulations of certain national institutions still prescribe to authors practices

at variance with the *Règles*. The only hope for the future therefore lies in simple, clear and objective rules, such that, even to those uninterested in nomenclature, they appear reasonable, secure and straightforward. The effect of a " parataxonomy ", whatever its safeguards and qualifications, would be to demolish the basic principle of " one animal, one name ", to introduce into nomenclature a system that is zoologically ridiculous, to confuse the student and to provide an excuse to the industrious but unintelligent for multiplying names of " parataxa " without making every effort to discover the zoological truth before burdening science with names. Any new system that allows authors to multiply names that will for ever be attributed to themselves will be abused.

10. Finally the fact that a few authors have proposed systems contrary to the *Règles* and to zoological common sense, need not and should not be taken to mean, as is implied by Moore and Sylvester-Bradley, that their schemes are " in current use ". The great system of aptychus names stems mainly from one author, Trauth, and is entirely rejected by the three contributors to the Mesozoic part of the Ammonoidea volume of the *Treatise on Invertebrate Paleontology* (Arkell, Kummel and myself). There is no desire so far as I am aware among the five living specialists on Mesozoic Asteroidea to erect a parataxonomy for isolated asteroid ossicles, nor am I aware that any living Echinoid specialist wishes to erect a parataxonomy for unidentifiable fragments or parts of echinoids. In fact I can only conclude that Moore and Sylvester-Bradley are proposing this major disturbance to the present system of zoological nomenclature primarily because of the very real difficulties in the small case of conodonts and the potential but easily removable difficulties in the equally limited case of ammonite aptychi.

11. I therefore advise that Moore and Sylvester-Bradley's proposals be rejected and that particular solutions be found within the *Règles* for particular problems.

DOCUMENT 1/50

Comment on the Moore/Sylvester-Bradley " Parataxa Plan "

By **M. K. HOWARTH**

(*British Museum* (*Natural History*), *London*)

(Enclosure to a letter dated 11th December 1957)

While supporting in general the aims and purpose of the proposal to recognise Parataxa, I believe it is essential for the Commission to consider the theoretical implications that would be enhanced by recognition of this category. For the first time the Commission is proposing to depart from the whole foundation and basis of Zoological Nomenclature and give validity to certain binomial names which in some cases do not conform to any possible definition of the most fundamental unit of nomenclature, the species. Thus, when several " species " and " genera " of conodonts are found together in a distinct symmetrical orientation in a conodont " assemblage " (almost certainly the remains of a single invididual) it becomes objectively provable that those " species " and " genera " are, in fact, conspecific. Yet it is proposed to give validity to these " species " and " genera " as Parataxa ; clearly this extension of the concept of Zoological Nomenclature warrants the closest attention. It is implicit in the plan (as it now stands) that species and genera of Parataxa are to be considered as of the same kind as species and genera of ordinary taxa, for Taxa and Parataxa are to be united for purposes of Homonymy. Thus the plan will tend to foster the idea that species of Parataxa, being of the same kind as species of Taxa, have the same attributes as species of Taxa ; i.e. they can be used in the same way for correlation and evolution. It is, however, well established that neither is true : similar aptychi (congeneric at least) are known in association with ammonite conchs belonging to different families, and in some cases evolutionary lineages for ammonites drawn up on their aptychi alone can be proved incorrect by reference to their associated conchs ; in long distance correlation by means of conodont " faunas " the only unit that can safely be used is the association of species or " species list ", not the single individual " species " which in some cases is known to be a component of several conodont assemblages representing different species and genera. Therefore on theoretical grounds it is indefensible to propose full validity for species and genera of Parataxa.

2. From the point of view of practical palaeontologists it is essential to stabilise the relationship between nomenclature of certain Taxa and their Parataxa, and to give validity or legality to names in both categories. The Parataxa plan is the best way to achieve this stability, with names co-ordinate for homonymy but separated for priority purposes. However, I would strongly urge the Commission to insert into the proposals a statement to the effect that " even though nomenclature of Taxa and Parataxa are co-ordinate for purposes of homonymy, in Parataxa the groups are as paraspecies and paragenera ". This will then imply that in Parataxa these groups are separated in kind from species and genera of Taxa, so that the Commission will not be giving validity to " species " of Parataxa which in some cases are provably conspecific. It will also enable paraspecies and paragenera to be correctly and validly put into synonymy under their true species and genera, if and when these are discovered, thus promoting the use of these species and genera for correlation and evolution. It is to be understood that even though several paraspecies and paragenera can be validly put into synonymy under one species, they still remain valid as paraspecies and paragenera. In this way these categories in Taxa and Parataxa will be separated in kind.

3. The terms " partial-species " and " partial-genus " have recently been proposed by Müller (1956, *J. Paleont.* **30** : 1324–40) for conodont nomenclature. Unfortunately these imply that the categories they represent are necessarily smaller divisions than species and genera, and they cannot, therefore, be used generally for all Parataxa (e.g. ammonoid aptychi). Paraspecies and paragenera can be applied generally, they are non-commital as to relationship between nomenclature of Taxa and Parataxa, and they are linked etymologically with Parataxa.

DOCUMENT 2/18

(*continued from page* 254)

Proposed maintenance of Copenhagen Decision 54(1)(a)

(Commission Reference : Z.N.(S.) 835)

By **R. I. SAILER**

(*U.S. Department of Agriculture, Entomology Research Branch, Beltsville, Maryland, U.S.A.*)

(Letter dated 19th December 1957)

In Volume 15, Double-Part 5/6 of the *Bulletin of Zoological Nomenclature*, I note evidence of what might be interpreted as a ground swell of opinion against Decision 54(1)(a) of the Copenhagen Congress. The opposing views, apparently stimulated by Dr. Arkell's strongly worded argument, are obviously based on the assumption that this decision would be retroactive. Evidently the members of the Colloquium failed to anticipate this regrettable interpretation of the decision. Surely this problem can be resolved by simply adopting the section of Bradley's draft pertaining to Article 5 that states, "No new rule shall retroact in such a manner as to overturn the well-established usage of any name".

This will leave Decision 54(1)(a) intact and provide zoologists with a rule that will eliminate the most important cause of future instability in names for those taxons between the genus and the superfamily.

DOCUMENT 2/19

Proposed maintenance of Copenhagen Decision 54(1)(a)

(Commission Reference : Z.N.(S.) 835)

By **JAMES A. SLATER**

(*University of Connecticut, Storrs, Connecticut, U.S.A.*)

(Letter dated 30th December 1957)

May I express my strong feeling against any attempt to weaken, change or eliminate Copenhagen Decision 54(1)(a). This decision is a definite and firm statement that cannot help but to bring about as great a degree of stability in family-group names as is possible.

2. Opposition to this decision appears to be very short sighted and to miss the spirit for which the rules are in existence. Furthermore the arguments of such persons as Dr. Arkell appear to lack understanding of the, as I believe, retroactive clause involved.

3. I sincerely hope that no hasty action will be taken in this situation. Such a Decision should have been in the rules a generation ago, it would be folly to lose such stability once it has been attained.

DOCUMENT 13/4

(*continued from page* 272)

Comment on the proposed adoption of a " Declaration " to treat barbarous words as exempt from change in gender

(Commission Reference : Z.N.(S.) 1064)

By **CYRIL F. DOS PASSOS**

(*Mendham, N.J., U.S.A.*)

(Letter dated 16th December 1957)

While it is unfortunate that so many modifications and exceptions to the *Règles* as amended at Paris (1948) and Copenhagen (1953) are being proposed, it appears that this *Declaration* is necessary, and the only question in my mind is whether subdivision (2) goes far enough, whether both subdivisions could not be consolidated and whether another matter should not be covered at the same time.

It is noted that subdivision (2) relates only to compound words where that word is adjectival in form and its final component is wholly Greek in form and origin. Why not when its final component is wholly " barbarous " in form and origin ?

In my opinion a further subdivision would be desirable. As Professor Grensted has pointed out in the Appendix, there are many scientific names the origin of which is unknown, uncertain or difficult to ascertain. *Erebia melas* is pointed out as one example, and Professor Grensted has *assumed* that Melas was taken from classical mythology, but who knows ? To solve problems such as this I suggest an amendment to the *Règles* to be incorporated wherever it may be deemed most appropriate in words or substance as follows :

> Where an author in proposing a scientific name fails to indicate its origin, that name shall be presumed conclusively to be Latin or Latinized and if necessary shall be amended in accordance with the other appropriate provisions of the *Règles*.

DOCUMENT 13/5

Comment on the proposed adoption of a " Declaration " to treat barbarous words as exempt from change in gender

(Commission Reference : Z.N.(S.) 1064)

By **G. VAN SON**

(*Transvaal Museum, Pretoria, South Africa*)

(Letter dated 3rd December 1957)

Many thanks for the five separates, references Z.N.(S.) 1064 (position as regards specific names consisting of partially Latinized words) . . . sent to me for commenting upon.

I completely agree to the proposed recommendations concerning each one of the items dealt with, in their entirety, and hope they will be sanctioned by the International Commission.

DOCUMENT 14/3

(*continued from page* 284)

Proposed amplification of the " Code of Ethics "

(Commission Reference : Z.N.(S.) 763)

By CYRIL F. DOS PASSOS

(*Mendham, N.J., U.S.A.*)

(Letter dated 16th December 1957)

This will advise you that I am in accord with the proposed adoption of a *Declaration* clarifying and extending the provisions of the " Code of Ethics " as set forth on page 176 of the 1957 *Bulletin of Zoological Nomenclature* **13** : 176.

Your revised text of the " Code of Ethics " is a great improvement on the text adopted as Paris in 1948 (1950, *Bull. zool. Nomencl.* **4** : 167).

DOCUMENT 14/4

Proposed amplification of the " Code of Ethics "

(Commission Reference : Z.N.(S.) 763)

By **G. VAN SON**

(*Transvaal Museum, Pretoria, South Africa*)

(Letter dated 3rd December 1957)

Many thanks for the five separates, references Z.N.(S.) 763 (Code of Ethics) . . . sent to me for commenting upon.

I completely agree to the proposed recommendations concerning each one of the items dealt with, in their entirety, and hope they will be sanctioned by the International Commission.

17 FEB 1958

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THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

PURCHASED
- 6 MAR 1958

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Sixth Instalment of the London Congress Agenda Paper

Case No. 24 : Points of Difference between the Draft of the English Text of the " Règles " and existing Congress Decisions : Report by the " Règles " Section of the International Trust for Zoological Nomenclature

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INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

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Secretary: Mr. Francis HEMMING (*London, England*) (27th July 1948)

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(*Arranged in order of precedence by reference to date of election or of most recent re-election, as prescribed by the International Congress of Zoology*)

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Professor Enrico TORTONESE (*Museo di Storia Naturale "G. Doria", Genova, Italy*) (16th December 1954

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* Professor Esaki died on 14th December, 1957, while the present Part was passing through the press.

BULLETIN OF ZOOLOGICAL NOMENCLATURE

Volume 15, Quadruple-Part 12/15 (pp. 357—488) 18*th February* 1958

PROVISIONS IN PROFESSOR CHESTER BRADLEY'S DRAFT OF THE "REGLES" NOT COVERED BY CONGRESS DECISIONS OR BY "DECLARATIONS" SUBSEQUENTLY ADOPTED

Statement prepared by the International Trust for Zoological Nomenclature

In the note prefixed to Professor J. Chester Bradley's Draft of the English text of the *Règles* attention was drawn (paragraph 4) to the fact that at various points in that Draft Professor Bradley had either included provisions not covered by existing Congress Decisions or by *Declarations* subsequently adopted by the International Commission or for reasons explained in the Draft had included provisions differing from existing Congress Decisions. It was then intimated that it was proposed to publish as Part 10 of Volume 14 of the *Bulletin of Zoological Nomenclature* a Report prepared by the "*Règles*" Section of the Office of the Trust, in which the differences referred to above had been briefly enumerated. At the same time it was added (paragraph 6) that it was proposed also to incorporate in Volume 15 of the *Bulletin* appropriate references to the points specified in the foregoing Report, in order that, as arranged with the authorities of the London Congress, that Congress should have before it in a single volume a complete enumeration of the proposals for the further reform of the *Règles* which it is to be asked to consider (see 1957, *Bull. zool. Nomencl.* **14** : 3—4).

2. Further consideration has since been given by the Trust to the procedure to be adopted for giving effect to the decisions set out above. In consequence the Trust has now taken the view that it would better serve the general convenience—and in addition, would involve a lesser expenditure—if the Report referred to above were to be published in the present volume (Volume 15) instead of in the volume (Volume 14) in which the actual Draft of the English text of the *Règles* has been published. In accordance with this decision, the Report by the "*Règles*" Section has been placed on the London Agenda

Paper as Case No. 24 and is being published in the present Quadruple-Part (Part 12/15). A note explaining the change in procedure set out above will be included in the next Part of Volume 14 to be published.

3. The Report now published contains particulars of two hundred and fifty separate matters affecting the wording of the Draft of the *Règles*, each of which will require to be considered separately by the Colloquium and the Congress in London. It is evident therefore that this Report will become the basic commentary on the London Agenda Paper to which all other comments on Professor Bradley's Draft will need to be related.

4. The Trust is of the opinion therefore that the discussions at the London Congress will be facilitated if the above Report is printed in such a way as to enable members of the Colloquium and the Congress to annotate their copies by inserting at appropriate points notes of the Case and Document Numbers of papers published in other Parts of the present volume. It has accordingly been decided to print the Report across left- and right-hand pages as a double spread and to leave a substantial space between each of the two hundred and fifty items concerned. For the further convenience of members of the Congress making notes either of documents published elsewhere in the present volume or of other relevant matters, a black rule has been inserted between each item.

5. It is already evident that the number of items to be considered in the course of examining the Draft of the English text of the *Règles* will be very large and will tax to the utmost the energies both of the Colloquium and of the Congress. It is therefore particularly hoped by the Trust that in advance of the London meetings zoologists and palaeontologists who propose to attend the Colloquium will annotate their copies of the present Part in the manner suggested in paragraph 4 above, thus assisting to some extent in the avoidance of unnecessary delays when the discussion of the Draft actually takes place.

FRANCIS HEMMING

Managing Director and Secretary,
International Trust for Zoological Nomenclature

6*th December* 1957.

CASE No. 24

PROVISIONS IN PROFESSOR CHESTER BRADLEY'S DRAFT OF THE ENGLISH TEXT OF THE "REGLES" NOT COVERED BY CONGRESS DECISIONS OR BY "DECLARATIONS" SUBSEQUENTLY ADOPTED BY THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

DOCUMENT 24/1

PROVISIONS INCLUDED IN PROFESSOR J. CHESTER BRADLEY'S DRAFT OF THE ENGLISH TEXT OF THE " REGLES " WHICH ARE NOT COVERED BY EXISTING CONGRESS DECISIONS OR ARE AT VARIANCE WITH CONGRESS DECISIONS

Report by the " Règles " Section, Office of the International Trust for Zoological Nomenclature

In accordance with the instructions given by the International Trust for Zoological Nomenclature at the time of the establishment of the " *Règles* " Section of its Office, the Draft of the English text of the *Règles* prepared by Professor J. Chester Bradley has been carefully compared with the text of the *Règles* as it existed up to the time of the Paris Congress in 1948 and with the amendments and additions made in that text both by the Paris Congress and by the Copenhagen Congress in 1953. It has been found that in almost every case Congress Decisions have been re-arranged or re-worded. In the present Report an attempt has been made to draw attention only to the more important changes noted.

2. In the light of the foregoing survey a list has been prepared in which brief particulars are given of each passage in the Draft of the *Règles* where the provisions inserted are either (1) provisions not covered by Congress Decisions or (2) provisions which are at variance to a greater or less extent with Congress Decisions. The number of items comprised in this list is two hundred and fifty (250). The list so prepared is given in the Appendix to the present Report.

ANN WILSON

Research Assistant in Charge, " Règles " Section, International Trust for Zoological Nomenclature

6th October 1957.

APPENDIX TO DOCUMENT 24/1

Comparison of the Draft of the English text of the " Règles " with the relevant Congress Decisions and " Declarations " rendered by the International Commission on Zoological Nomenclature

For purposes of convenience the following abbreviations are used in the present Appendix :—

(1) The word " Berlin " followed by an Article number is a reference to the English text of the *Règles* as compiled by the Fifth International Congress of Zoology, Berlin 1901, and as amended by successive Congresses up to and including the Twelfth International Congress of Zoology, Lisbon 1935. No official text of the *Règles* as they existed up to the eve of the Paris Congress in 1948 was ever published. The following unofficial (but substantially correct) English texts may be consulted :—

> " International Rules of Zoological Nomenclature " published in *Règles Internationales de la Nomenclature Zoologique adoptées par les Congrès Internationaux de Zoologie*, Paris 1905 [exclusive of amendments made by later Congresses]
>
> " International Rules of Zoological Nomenclature " published in 1926 in *Proceedings of the Biological Society of Washington* Vol. **39** : 75–104

(2) The word " Paris " followed by a page number is a reference to the Official Record of the decisions taken by the Thirteenth International Congress of Zoology, Paris, 1948, published in 1950 in Volume 4 of *The Bulletin of Zoological Nomenclature.*

(3) The word " Copenhagen " followed by a page number is a reference to the Official Record of the decisions taken by the Fourteenth International Congress of Zoology, Copenhagen, 1953, published as *Copenhagen Decisions on Zoological Nomenclature*, 1953.

(4) The word " *Declaration* " followed by a number is a reference to a *Declaration* adopted by the International Commission on Zoological Nomenclature and published in various volumes of *Opinions and Declarations Rendered by the International Commission on Zoological Nomenclature.* The reference " B " followed by a volume and page number refers to the volume of *The Bulletin of Zoological Nomenclature* in which a proposed *Declaration* under consideration by the Commission is to be found.

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(1)	Introduction	Paris : 166—Conclusion 50
(2)	Preamble	Copenhagen : 22—Decision 19
(3)	I/Foreword	
(4)	1/1	Paris : 144—Conclusion 16

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
insertion of draft defining the force of the terms used in the *Code*; incorporates part of *Declaration* 9	*Declaration* 9 was considered unsuitable for embodiment in the *Code* at Paris
re-words	
paragraph by Compiler explaining the system to which zoological nomenclature applies	
re-drafts provision on what a name designates	based on part of *Opinion* 2 which was not incorporated in the *Code* at Paris

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(5)	1/2	Copenhagen : 63—Decision 114
(6)	1/3	Paris : 255—Conclusion 18(1)
(7)	1/4	Paris : 364—Conclusion 10 Copenhagen : 63—Decision 113
(8)	1/5	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
re-words with qualifying phrases introduced from other parts of the *Code* (old Articles 2 and 3)	
adds a provision that the name of an animal based on its work is only an available name if published before 1931	see items (21, (38), (39) and (51) below
re-words the Paris Decision on names given to monsters and re-drafts in a positive form the negative Copenhagen Decision on names based on specimens later regarded as pathological monstrosities	gives as reference the roneoed draft of the Colloquium Report (Copenhagen MSS. 31 paragraph 75) [the wording in the Report as approved and published is similar to that in the roneoed draft, except that the last sentence, on the Colloquium being against any provision involving a subjective test of this kind does not occur in the roneoed draft]
new provision explicitly stating that the same system of zoological nomenclature applies to extinct as well as to living animals	suggested by Professor E. Mayr

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(9)	4/5	
(10)	5/1	
(11)	5/2	Paris : 56—Conclusion 7(2) : 292—Conclusion 10(6)
(12)	5/3, 4	Copenhagen : 25—Decisions 27–29

Nature of difference from Congress Decision or from subsequent " Declaration	**Notes by " Règles " Section**
provision not previously stated on the position of a work infracting old Article 2 (the number of words in scientific designations of animals)	Compiler states this is added for completeness and clarity
new provision on the retroactive application of the Rules	suggested by Professor Blake, based on the legal principle of "*stare decisis*"; Compiler—"It appears to have been accepted but not formulated at Copenhagen" [see also item (36)]
formulates specific statement on actions which may be taken under Plenary Powers and adds a reference to the regulations governing the use of these Powers	Compiler notes that the sentence that the Plenary Powers are subject to certain regulations and restrictions (as in the procedure to be followed in voting on cases involving Plenary Powers) is not explicitly stated by any Congress enactment, but that it is the practice; in addition the Compiler inserts a definition of the purposes of the Plenary Powers which has the effect of restricting the scope of the decision taken by the Paris Congress [see item (232)]
new draft of a provision on the Principle of Conservation and the procedure necessary to conserve names under it	wording not agreed at Copenhagen but a Directive issued that such a provision to be included in terms to be agreed upon by the Commission

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(13)	5/4	Copenhagen : 25 —Decisions 27–29
(14)	5/5(a)	Copenhagen : 24—Decision 26
(15)	6/1	Berlin. Article 25 Paris : 65—Conclusion 3 Paris : 69—Conclusion 6 Paris : 72—Conclusion 8 Paris : 175—Conclusion 67
(16)	6/1(j)(2)	Berlin. Article 25(c) [added at Budapest 1927] Paris : 69—Conclusion 6(1) Copenhagen : 61—Decision 109

Nature of difference from Congress Decision or from subsequent “ Declaration ”	**Notes by “ Règles ” Section**
formulates as a definite Rule the procedure to be followed when the Commission is notified of a long-overlooked name and suggests limiting the Rule to those names which are a potential threat to stability ; adds a provision on the action to be taken if an objection is received	Compiler’s provision appears to be based on a Paris enactment (Paris : 234—Conclusion 4) rather than on the Copenhagen decisions given in reference ; it is not clear whether the suggested Copenhagen Draft is intended to replace this Paris enactment, on which *Recommendations* in the previous section [Draft 5/3] are also based causing partial duplication as in *Recommendations* 2 and 5
omits the word “ specific ” to widen the application of the provision on the suppression of *nomina dubia*	Compiler’s comment—“ A *nomen dubium* may be a generic name . . . even . . . the name of a higher taxon ”
complete re-organisation of old Article 25 (Law of Priority) into Article 6 “ The Rules of Availability ”, Article 7 “ What Constitutes Publication ”, Article 8 “ The Principle of Priority : The Validity of Names ” and Article 9 “ Date ” ; old Article 25 as amended at Paris and Copenhagen re-worded to fit in with this re-organisation	the distinction between an “ available ” name and a “ valid ” name [defined at Paris : 336, Conclusion 21(4)] is made in Draft 6/1 Explanation and Draft 8/Foreword ; the definition of the terms adopted at Paris are not included in the Draft
restores the wording deleted by the Paris Congress of “ a definite bibliographic reference ” which occurred in one of the conditions which conferred availability on a name published after 1930	the Budapest Congress’s phrase “ a definite bibliographical reference ” which is retained by the Compiler was deleted by the Paris Congress on account of the strong criticisms which had been levelled against it on account of its “ ritualistic ” character ; the provision in the Draft is not limited to substitute names which alone were considered by the Paris Congress [see item (17)] and the point noted above does not apply therefore to cases where substitute names are not involved

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(17)	6/1(j)(3)	Paris : 69—Conclusion 6(1)(a)
(18)	6/1(1) and 6/9(c)	Paris : 214—Conclusion 14
(19)	6/2	Paris : 309—Conclusion 2(1)
(20)	6/3(2)	Paris : 149—Conclusion 21(a)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
deletes wording " invalid by reason of being a homonym ", which in the Paris provision qualified the name which is to be replaced	Compiler comments that the important point is that the name was proposed as a substitute and not the reason and that to prove such invalidity should not be necessary to make the replacement name available [see item (20)]
widens the provision on names published anonymously to apply to names in general	applied at Paris to family-group names and names of lower taxa
re-words, altering the emphasis of, the provision on the status of a work dealing only with genera or higher taxa and not mentioning a binomen	
omits words qualifying the names to be replaced as an " invalid homonym "	Compiler believes that the provision " should apply to all replacements " [see Draft item(17) above]

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(21)	6/3(3)	Paris : 255—Conclusion 18(1)
(22)	6/5(a)	Paris : 152—Conclusion 26(1)
(23)	6/5(c)	Paris : 144—Conclusion 17
(24)	6/5(e)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
limits the provision on the work of an animal counting as an indication to apply only to names published before 1931	see items (6), (38), (39) and (51)
mentions specifically that the provision on composite nominal species also applies to subspecies and infra-subspecific forms	
re-drafts the provision on the status of names published conditionally	
adds at the end a sentence further clarifying the provision on names based on part of an animal	Compiler deems this inherent in old Article 27

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(25)	6/6(b)	Copenhagen : 65—Decision 120
(26)	6/7	Copenhagen : 63—Decision 115
(27)	6/7(c)	Copenhagen : 63—Decision 115 (1)
(28)		Copenhagen : 64—Decision 116

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
re-drafts provision on names intended for use exclusively for fossils, re-arranging points, and adds an " Explanation "	
re-arranges the points in the provision on names published in synonymies ; amplifies to clarify and to take into account the case of the original nominal species being misidentified	Professor Mayr believes that the Report as approved and published did not succeed in representing the consensus of the Copenhagen Colloquium on names published in synonymy
changes part of the provision on the unavailability of a name published without " an independent indication ", etc., to " without the required data "	Compiler states that the Copenhagen wording does not give all the requirements necessary to make a name published after 1930 available
omits *Recommendation* against bringing into use, before the coming into force of the provision making such names unavailable, names published in synonymies which are not already generally accepted	Compiler expresses the view that this *Recommendation* is contrary to the Copenhagen Decision that such a name unless it has already been brought into general use is not available (Copenhagen : 64—Decision 115[2])

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(29)	6/9(a)	Paris : 150—Conclusion 24
(30)	6/9(b)(i) and 6/9(b)(ii)(4)(5)	Paris : 145—Conclusion 18 Copenhagen : 64—Decision 115
(31)	7/2(a)(i)	Paris : 217—Conclusion 15(1)(a)(i)(α)
(32)	7/2(b)(2)	Paris: 218—Conclusion 15(1)(a)(ii)(γ)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests the addition of subsequent " citation in synonymy " as another action which does not suffice to make a name published before 1758 available	
restores some of the wording deleted by Copenhagen, in the Paris provision on the status of a manuscript name first published with an "indication"	Copenhagen Decision 115 does not make it quite clear that it only replaces the part on manuscript names published in synonymy without an independent " indication "
introduces words " large-scale " to qualify the type of reproduction necessary to constitute " publication "	Compiler adds this qualification to exclude a few carbon copies from counting as a " publication "
omits the opening qualification that the provision on the requirement for sale or distribution applies only where the author distributes the document to certain selected persons	Compiler holds the view that his omission results in a more general statement which expresses the intent at Paris rather than the narrow wording used

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(33)	7/4	Paris : 219—Conclusion (15)(I)(b) Copenhagen : 61—Decision 108
(34)	7/4(6)	Paris : 146—Conclusion 19(b) Paris : 219—Conclusion 15(I) (c)(ii)
(35)	7/4(6) Second sentence	
(36)	8/1	Berlin. Article 25 Paris : 130—Conclusion 6

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
re-drafts the provision on what does not constitute publication to cover the Copenhagen Decision	
	suggestion by Mr. dos Passos that the provision that a separatum is not counted as published until the publication containing it is issued, should only apply after a certain date
suggests the addition of a new provision to protect the status of a separatum that was to be in a publication which subsequently was not published	
re-drafts, in accordance with the re-arranged Articles, the provision on the application of the Law of Priority ; includes in the provisos limiting the Law reference to Draft Article 5 (Continuity and Universality of Usage) ; the form of the draft includes additional " Limitations " and an " Explanation "	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(37)	8/2 *Recommendation*	Copenhagen : 67—Decision 125
(38)	8/2(b)(6)	Paris : 255—Conclusion 18(1)
(39)	8/2(b)	Paris : 255—Conclusion 18(2)
(40)	9/Foreword	

Nature of difference from Congress Decision or from subsequent " Declaration"	Notes by " Règles " Section
states specifically that a taxonomist acting as a First Reviser should if possible select the name that would best preserve stability	
adds date, 1930, to limit the period in which the description of the work of an animal constitutes an " indication "	Compiler makes this addition as otherwise states that this provision would be inconsistent with the further particulars required as an " indication " after 1930 [see also items (6), (21), (39) and (51)]
places as doubtful the *Recommendation* against basing a name solely upon the work of an animal	this *Recommendation* would be superfluous in the light of the Compiler's interpretation given above [item (38)]
adds introductory drafting to the Article on date of publication	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(41)	10/Foreword	
(42)	10/1 *Recommendation* 4	Copenhagen : 62—Decision 110
(43)	10/1 *Recommendation* 5	Paris : 169—Conclusion 54
(44)	10/1 *Recommendation* 6	Paris : 126—Conclusion 2(2)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
drafts explanatory introduction to a new Article enumerating what a taxonomist must do when establishing a new taxon or name	
widens the *Recommendation* on the desirability of giving a taxonomic comparison for new genera and species to apply to all taxa	proposal is not altogether in accordance with the *Recommendation* adopted in regard to family-group names [Copenhagen : 35—Decision 52] ; Copenhagen Decision 110 replaces Paris : 71—Conclusion 7 which Compiler nevertheless restores [see items (47) and (49)]
suggests that, in the provision indicating the method of signalising a new name, the part on inserting a comma should be omitted ; applies to names in general	states that the Paris Decisions were inconsistent in regard to the use of a comma [see item (50)] ; applied at Paris to family-group names and names of lower taxa
widens provision on publicity for new names for family-group or lower taxa to apply to all taxa	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(45)	10/1 *Recommendation* 9	Berlin. Article 36
(46)	10/1 *Recommendation* 11	Copenhagen : 62—Decision 112(1)
(47)	10/1 *Recommendation* 13	Paris : 71—Conclusion 7(2)(a)
(48)	10/1 *Recommendation* 14	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
	discusses omission of the example " *macrodon* and *microdon* ", actually this was deleted by the Congress at Graz (1910)
suggests addition of words " if possible " to the *Recommendation* on selecting as a type species a species with a satisfactory figure	
restores the Paris form of the *Recommendation* on the desirability of giving a comparative description indicating characters that separate a new genus from the most closely related previously established taxon	the Paris *Recommendation* was replaced at Copenhagen by Decision 110 [item (42)]
suggests new examples to be added to *Recommendation* on the avoidance of similar specific names in the same or a related genus	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(49)	10/1 *Recommendations* 18 and 19	Paris : 71—Conclusion 7(2)(b)(c)
(50)	10/1 *Recommendation* 22	Paris : 92—Conclusion 1(8)(a)
(51)	10/2(b)	Paris : 255—Conclusion 18
(52)	10/2 *Recommendation* 28	Paris : 223—Conclusion 17

Nature of difference from Congress Decision or from subsequent “ Declaration ”	**Notes by “ Règles ” Section**
suggests omission of the part on the case of the most closely related species, if little known, not being used for comparison in the description of a species	but new draft substituted at Copenhagen [item (42)] [see also item (47)]
inserts for consistency the part on placing a comma before an expression indicating that a subspecies is new, but suggests this part should be omitted	Paris statements regarded by Compiler as inconsistent, compare Paris : 92, Conclusion 1(8)(a) and Paris : 169, Conclusion 54 ; see above item (43)
new provision that it is no longer sufficient to base the description of a new species solely on the work of an animal	see items (6), (21), (38) and (39) above
adds, to the *Recommendation* which advises against publication of names in abstracts in advance of their description, an “ Explanation ” defining the status of the names so published	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(53)	10/2 *Recommendation* 29	Paris : 253—Conclusion 16
(54)	11/1	Copenhagen : 43–46—Decisions 71–76
(55)	12/1(a)	Copenhagen : 38—Decision 62
(56)	12/1(a)(i)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
widens the provision on hidden meanings from applying to generic and specific names to apply to names in general ; suggests the omission of the phrase on an arbitrary combination of letters	
re-drafts and reorganises the provisions on the emendation of names ; the definitions of expressions introduced at Copenhagen are placed in a " glossary " at the end of the *Code*	Section 1 as drafted applies to names in general (not to generic and specific names as at Copenhagen) and is accordingly not consistent with the Copenhagen Decision on family-group names reproduced in Section 2 of Article 11
new provision on the status of names of taxa of the Phylum and Order/Class-Groups after the adoption of the *Official Lists* concerned	Compiler deems this interpretation to be inherent in the Copenhagen resolution
suggests new provision on categories not provided for in the *Lists* proposed for names in the Phylum and Order/Class-Groups	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(57)	12/2	Copenhagen : 40—Decision 63
(58)	12/3(a)	Copenhagen : 41—Decision 65
(59)	13/1(a)	Copenhagen : 32—Decision 44
(60)	13/2(b)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests adding sentence on the Commission deciding the case if a protest is received on the proposed adopting of a junior name on the union of two taxa of a rank above superfamily	
adds to the provision on the names that may be used when a taxon above superfamily is changed in rank the proviso that this does not apply when the taxon is lowered in rank into a category of the family-group	
suggests addition of sentence, to the provision on categories available for use in the family-group, that supplementary categories may be used when required	
new provision applying the stem provision to names of taxa of lower categories into which a family-group taxon may be divided	Compiler expresses the view that this is necessary for completeness and to conform to custom

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(61)	13/3(c)	Copenhagen : 36—Decision 53
(62)	13/4(d)	
(63)	13/5	
(64)	14/Foreword	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
suggests that the part providing for the retention of vernacular words for family-group names in certain cases, must be revoked	Compiler expresses the view that this part violates the requirement that a name be Latin or Latinised and that particular cases can be handled under Plenary Powers
adds provision not specifically stated before on altering a family-group name to conform with the spelling of the type genus if the latter required automatic correction	Compiler believes that this was not an intentional omission
adds provision not previously stated on substituting the appropriate suffix for a category if the original one inappropriate	Compiler considers that the meaning was implicit although not directly stated
adds opening draft drawing attention to the subjective taxonomic nature of the question of whether a taxon ranks as a genus or subgenus	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(65)	14/1(a)	Berlin. Article 7
(66)	14/1(b)	Berlin. Article 9 Copenhagen : 21—Decision 18
(67)	14/2	Berlin. Article 8 First *Recommendation* Copenhagen : 47—Decision 78(2)
(68)	14/5	Berlin. Article 10

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
additional " Explanation " clarifying the provision on the interchangeability of rank between genus and subgenus	
re-drafts the provision on a nominate subgenus adding an " Explanation "	
does not insert in *Recommendation* form the existing *Recommendation* on certain collective biological groups being treated as genera ; suggests adding the sentence that the names for such groups do not enter into zoological nomenclature and qualifying these groups by the re-drafted wording " not taxonomic in character "	according to Copenhagen : 47—Decision 78(2) this is restored to its original status of a *Recommendation* ; [the future of this provision is bound up with the decision to be taken by the Congress in Case No. 1 on the Agenda Paper]
re-drafts in amplified form the provision on the citation of a subgeneric name	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(69)	14/5 *Recommendation*	
(70)	15/footnotes	
(71)	15/1(e)	Paris : 191—Conclusion 2(1)
(72)	15/2(a)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
new *Recommendation* against citing any term other than that of a subgenus between the generic and specific elements of a binomen	suggested by Dr. K. P. Schmidt
footnotes added to explain the plan of the re-organisation of old Articles 8 and 14	
re-drafts, altering the form of, the provision on nominate subspecies	
new provision on the status of a taxon to which the term " variety " was applied before 1951	amendment proposed by Dr. Sabrosky ; this proposal is incompatible with the distinction in the *Regles* between subspecific and infrasubspecific names [Paris : 91 Conclusion 1(7)(a)]

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(73)	15/2(b) 15/3(a)	
(74)	15/2(c)	Copenhagen : 84—Decision 166
(75)	15/5(a)	Paris : 262—Conclusion 33
(76)	15/7(c)	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
new provision on the rank of a new taxon indicated as the form of a particular geographical area	suggested interpretation
adds, to the provision defining the status of a name published as a trinomen in the case of a Fossil species, a proviso excepting an author's express statement otherwise	
addition of an " Explanation ", in the form of a hypothetical example to the provision on the name of a nominal taxon on transfer in rank between a species and subspecies	
proposed new provision on the status of a subspecific or infra-subspecific name violating the rules for its citation	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(77)	15/8(a)	
(78)	15/8(b)	*Declaration* 30
(79)	15/8(c)	
(80)	16/1(a)	

Nature of difference from Congress Decision or from subsequent "Declaration"	**Notes by " Règles " Section**
additional matter on a specific name transferred to another genus than that with which it was originally established	Compiler adds for further clarification of *Declaration* 30 [see item (78) below]
re-words in a more concise form the provision on the generic name with which a species is first published when referred to an established genus and at the same time to a new conditionally established genus	
new provision on variant spelling or emendation of the generic or specific name not causing distinct binomina	the reference to a proposed " new *Declaration* " is not understood [see items (131), (137) and (159)]
new provision on the status of a name given to an individual " known " to be a hybrid	proposed by Dr. E. Mayr

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(81)	16/1(b)	
(82)	16/2	
(83)	17/Foreword	
(84)	17/(b)(2)	Copenhagen : 27—Decision 31

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
new provision on the status of a name given to a population believed to be hybrid or intermediate between two subspecies	proposed by Dr. E. Mayr
suggests the deletion of the provision that in the case of names for hybrids the name of the male parent shall precede that of the female parent	proposed on an observation by Dr. E. Mayr suggestion by Professor C. H. Blake that there should be provision for cases where the parentage is known but not the sex
adds opening explanation on the significance of the type-locality nomenclatorially and taxonomically	Compiler expresses the view that the appropriateness to the *Code* of this whole Article is questionable
new qualifying phrase on the portion of the originally cited area that may be selected by a first reviser	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(85)	17/(a)(b)(c)	Copenhagen : 26—Decision 31
(86)	18/Foreword	
(87)	18/1	
(88)	18/4	Copenhagen : 34—Decision 49(2)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
	gives as reference the roneoed draft of the Colloquium report (Copenhagen MSS. 31 paragraph 19 and MSS. 35 paragraph 5) as well as Copenhagen Decision 31 in the Report as approved and published
additional explanation on types	
new provision on the type of the nominate subtaxon	suggested by Dr. E. Mayr for completeness and clarity
additional explanatory matter in the form of examples on the relation of names of taxa, belonging to various categories within the family-group, to the name of the type genus	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(89)	18/4 *Recommendation* 1	Paris : 139—Conclusion 11(2)(b)
(90)	18/4 *Recommendation* 2	Paris : 139—Conclusion 11(2)(c)
(91)	19/Foreword	
(92)	19/2(b)(i)	
(93)	19/2(c)	Copenhagen : 69—Decision 128

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
retains the *Recommendation* advising that the genus selected as the type genus of a family should be well known	not re-enacted at Copenhagen and appears therefore to be cancelled by Copenhagen : 32—Decision 43
suggests elimination of the *Recommendation* providing for, in certain cases, the retention of a family-group name not based on its type genus	Compiler states that it contradicts the Copenhagen Decision that the basic stem of the name of each taxon of the family-group shall be that of its type genus (Decision 49) (see Draft 13/2 [a]). This provision which originally appeared in *Opinion* 141 and was cast in the form of a *Recommendation* at Paris appears anyway to have been cancelled by Copenhagen : 32—Decision 43
dditional explanation on the course f development of the type concept or genera	
uggests clause, on no change being ade that would upset usage, as a ew addition to the provision on rocedure to be followed if the pecies intended to be the type pecies is considered to have been isidentified	possibly inherent in Paris : 158—Conclusion 38(2)
-arranges and adds hypothetical xample to the provision on preiously misidentified type species	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(94)	19/3, 4	
(95)	19/3(a)(iii)	Copenhagen : 70—Decision 131
(96)	19/3(b)	Paris : 153—Conclusion 28 Copenhagen : 70—Decision 130 *Declaration* 26
(97)	19/3(d)	Paris : 154—Conclusion 29

Nature of difference from Congress Decision or from subsequent "Declaration"	**Notes by "Règles" Section**
the provisions relating to the method to be followed in determining the type species of a genus which appeared as "Rules" in the old Article 30 are here converted into subsequent provisions; the letters denoting the old "Rules" are altered and the order in which these provisions appear has been changed	original Rules (b) and (c) are reversed in order and original Rules (f) and (g) are placed in a seperate section and re-numbered
re-drafts the provision on the type species of a nominal genus established with more than one included nominal species but with the type species established only for a nominate subgenus	Compiler considers the part on the author not having established a type species for that genus already is misleading as it might be interpreted to mean that the type species of the genus might be differently designated, and this is impossible by definition
summarises in a redrafted form the classes of species that must be disregarded (as well as those unavailable) in the Rule on type species by monotypy	Compiler considers necessary for completeness
nserts the qualification "cited" or a synonym in the provision on ype by absolute tautonymy	Compiler considers that, although not in accordance with the Paris decision, unless this amendment is included a subjective element would be present

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(98)	19/3(d)(i)	Paris : 145—Conclusion 29
(99)	19/4(b)	*Declaration* 27
(100)	19/4(d)	Berlin. Article 30(g)
(101)	19/5(a)	Paris : 156—Conclusions 32 and 34

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
inserts the qualification " objective " for a synonym in the part on hidden tautonymy adding Examples to clarify the point ; suggests the omission of the reference to subspecies	
re-words and suggests adding new clause that the provision, clarifying the question of the type species of a substitute name, should apply despite any statement to the contrary by the author	
adds proviso that the type by subsequent selection may be reversed by Plenary Powers ; omits " valid " which qualified the originally included species	
mentions specifically, in the provision on species that are eligible for subsequent type selection, that *species inquirendae* and *species sedis incertae* are excepted	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(102)	19/5(c)(iii)	Paris : 180—Conclusion 69 (3)(b)
(103)	19/5 *Recommendation* 3(*v*)	Berlin. Article 30(t)
(104)	19/5 *Recommendation* 5	Paris : 126—Conclusion 2(3)
(105)	20/Foreword	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests additional sentence defining the date of type-selection when an author accepts a previous selection of a nominal species, not originally included, to be the type species and synonymises that species with an originally included species	
expands the page precedence *Recommendation* in selecting type species to a more definite statement mentioning line and word precedence	
the expression used by the Compiler is " recording publication "	the expression approved by the Paris Congress was " literature-recording serial "
new opening draft explaining the term " type "	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(106)	20/footnote	
(107)	20/1	
(108)	20/1 *Recommendation* 2	Paris : 188—Conclusion 75(7)(c)
(109)	20/1 *Recommendation* 3	Copenhagen : 30—Decision 37

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
addition of note on the term " lecto-holotype "	
addition of an " Explanation " interpreting the phrase " are the property of science "	
applies specifically the *Recommendation* on labelling of holotypes and lectotypes to labelling of syntypes and neotypes	
suggests adding a sentence on the reason for publishing information on labels when designating types of species	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(110)	20/2 *Recommendation* 8	Paris : 187—Conclusion 75(7)
(111)	20/3(a)(i)	Copenhagen : 73—Decision 137
(112)	20/3(a)(iv)	Copenhagen : 75—Decision 141
(113)	20/3(c)	Copenhagen : 74—Decision 139

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
suggests new wording qualifying the requirement that data on the stage of a holotype should be given, so as to exclude cases where the type is a mature adult	
adds qualifying clauses, to the provision on selecting a syntype to be a lectotype, which provide that such a selection is not final if (a) it is contrary to action by previous revisers and if (b) the type selected proved not to be a syntype	in its present form the suggested addition in (a), if approved, would be incomplete for there is no previous provision requiring action by a reviser
adds the point that the existence of only faultily preserved syntypes absolves a taxonomist from selecting a lectotype therefrom	see items (118), (237)
adds explanation on the effect of making a single specimen the holotype or lectotype of two nominal species	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(114)	20/3 *Recommendation* 10	Paris : 188—Conclusion 75(7)(b) Copenhagen : 77—Decision 148
(115)	20/4(a)	Copenhagen : 28—Decision 34
(116)	20/4(b)	Copenhagen : 28—-Decision 34(3)
(117)	20/4(d)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests sentence on the kind of collection from which a lectotype should be selected	
new sentence on the purpose of establishing a neotype	Compiler's suggestion to replace Copenhagen Decision 34(2), also gives alternate drafting of the Copenhagen version
re-drafts in an affirmative form the provision defining the class of species for which a neotype is to be established	
provision on priority if there are two neotypes established for the same nominal taxon	Compiler states that, according to Dr. Sabrosky, this provision was adopted by the Colloquium but not included in the Report as approved and published

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(118)	20/5(a)(iv)	
(119)	20/5(a)(ix)	Copenhagen : 29—Decision 35(6)
(120)	20/5 *Recommendation* 12	Copenhagen : 29—Decision 36
(121)	20/5(b)	

Nature of difference from Congress Decision or from subsequent “ Declaration	Notes by “ Règles ” Section
suggested new provision allowing for existing type material, if faulty, to be disregarded for neotype selection	see items (112) and (237)
re-words provision on the type of descriptive data that must be published on the establishment of a neotype	Compiler’s replacement of Copenhagen Decision 35(6)
qualifies the *Recommendation* on consultation before designating a neotype by adding the words “ if any ” in regard to fellow workers	
new provision on Commission’s Powers in regard to establishing a neotype	suggested by Dr. K. P. Schmidt

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(122)	20/5(e)	Copenhagen : 31—Decision 39(6)
(123)	20/5(a)(v)(vi)	Copenhagen : 29—Decision 35(5)
(124)	20/6	Copenhagen : 31—Decision 40
(125)	20/7 *Recommendation* 16	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
adds qualifications to the provision relating to the procedure to be followed on the final validation of a neotype	
re-arranges some of the points in regard to establishing a neotype	
omits " provisional " to make more definite the action of a First Reviser in regard to retaining or rejecting a neotype when part of the original type material is subsequently discovered	
new *Recommendation* on not changing the specimen selected for a neotype and published before the Article in question takes effect	suggested by the Compiler to guard against the work of an original selector being overthrown

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(126)	21/2	Copenhagen : 72—Decision 136
(127)	21/6	Berlin Article 29
(128)	22/Foreword	
(129)	22/1(b) and *Recommendation* 1	*Declaration* 32

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
adds " Explanations " to the provisions on the subjective and objective definition of a species	
re-drafts the provision of original Article 29 and omits the part on the necessity for the type species having been originally established for the nominal genus in question	Compiler states that in practice the Rule in original Article 29 is followed even when the type species was selected after the publication of the generic name
adds opening paragraph on the purpose of quoting the author of the name of a taxon	
re-words and condenses the provision on the status of names in unpublished papers presented at meetings	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(130)	22/1(f)	Paris : 144—Conclusion 17(a)
(131)	22/2(a)	
(132)	22/2(b)(ii)	Copenhagen : 59—Decision 105(2)
(133)	22/4(a)	Copenhagen : 37—Decision 56 : 42—Decision 69

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
suggests the omission of the word " specific " to broaden the application of the provision on the author of a name published conditionally	by the time of the publication of the Draft *Code* the action here proposed had already been taken in *Declaration* 24
new provision on variant spelling of generic or specific names not causing new combinations	the reference is not understood [see items (79), (137) and (159)]
omits " on subjective grounds " from the provision that a later author claiming that the author of a new combination misidentified the nominal species concerned does not affect the ruling concerning the authorship of such a combination	Compiler considers the omitted wording irrelevant
re-drafts the provision on the citation of the name of the author of a family-group name or higher, altering the emphasis and putting part as a *Recommendation*	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(134)	22/4(b)	
(135)	22/5 *Recommendation* 4 Second version	Paris : 215—Conclusion 14(2)
(136)	22/*Recommendations* 5 and 7	Paris : 174—Conclusion 65
(137)	22/5(c)(iv)	Berlin. Article 23

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
adds footnote on the custom in regard to the citation of the name of the author of a taxon of generic or lower rank	
suggests new *Recommendation* that the author of a name published anonymously, but later known, should be given and the abbreviation " anon " not used	proposed by Compiler in replacement of the *Recommendation* that an author's name in such a case should be placed in square brackets
retains the *Recommendation* against abbreviating the names of authors	Compiler raises the question whether this was cancelled by Copenhagen : 59—Decision 104
re-drafts opening wording of the provision on the citation of the author of a new combination and suggests addition of clause to exclude variant spellings of any of the names counting as a new combination	additional clause on variant spelling not traced [see items (79), (131) and (159)]

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(138)	22/5(d)	Berlin. Article 24
(139)	22/7 *Recommendation* 6	Paris : 169—Conclusion 55
(140)	22/7 *Recommendation* 9	Paris : 170—Conclusion 57
(141)	23/Foreword	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
re-drafts and adds " Explanation " of the significance of revising a species ; retains the example based on Goeze's action as a taxonomist	according to Paris : 174, Conclusion 64 this example was to be cancelled as Goeze was not a consistently binominal author
re-words in a condensed form the *Recommendation* on not signalising that a taxon is new more than once	
substitutes the word " taxonomist " for " author " in the *Recommendation* on giving a bibliographical reference to its original publication when citing the name of a genus or lower taxon	Compiler states that this re-wording is to remove popular articles from being covered by the provision
adds opening paragraph explaining the Article on " The Rejection of Names "	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(142)	23/1(a)(ii)	Paris : 242—Conclusion 3(2)
(143)	24/Foreword	
(144)	24/1	Berlin. Articles 34 and 35
(145)		
(146)	24/1(i)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests the omission of the phrase " or in group of allied genera ", as too broad, in the provision on the Commission's Powers in regard to similar compound specific names (differing only in being in noun or adjectival form)	Compiler states that Dr. Sabrosky suggests that the Rule should be broadened to include other cases of similarity ; Compiler suggests that provision might be rescinded anyway, as the Plenary Powers of the Commission would cover these cases
Compiler inserts an explanation of the principle of homonymy	
new draft of the Law of Homonymy broadened to apply to all taxa with the qualification of " based on a different type " introduced	as worded, this does not bring out clearly the position of family-group names and names of higher categories ; wording similar to that on being based on a different type, is used in all the drafts on homonymy [see items (153) (154) (156) and Draft 24/10(a)]
omits in this Article definitions of primary and secondary homonyms, although these phrases are used in this Article	(these definitions are inserted in the Glossary)
new provision on the Commission's Powers to conserve a junior homonym	Compiler states that this proposal was suggested to him by Dr. E. Mayr ; but that, anyway, the Commission has this right under its Plenary Powers; [see Paris : 339, Conclusion 23(1)]

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(147)	24/1 *Recommendation* 1	Paris : 119—Conclusion 1(4)
(148)	24/2(b)	Paris : 163—Conclusion 44 : 183—Conclusion 74(c)
(149)	24/2(c)	Paris : 398—Conclusion 35(4)
(150)	24/4(a)(b)	Copenhagen : 78—Decision 152 : 79—Decisions 154 and 155

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
widens the *Recommendation* on giving publicity to invalid specific homonyms to include all homonyms; and adds proviso that it does not apply when the author discovering such a homonym is himself taking steps to replace it	
widens the provision on emendations of specific and generic names entering into homonymy to cover names of all taxa	
condenses the provisions on the status of *nomina dubia* in regard to homonymy into a more general statement and omits Paris example	
inserts as a substantive provision, applying to names of taxa in general, the special cases (i.e. the variations on " Mac " and diacritic marks) of differences which are to be treated as causing homonymy	Compiler expresses the opinion that the Copenhagen Report as approved and published omits wording which occurred in the roneoed draft of the Colloquium report and which referred to differences in spelling to be ignored in regard to generic homonymy

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(151)	24/4(b)	Copenhagen : 79—Conclusion 155
(152)	24/4 *Recommendation* 3	Paris : 167—Conclusion 51
(153)	24/5(a)	Copenhagen : 42—Decision 68
(154)	24/6(a)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
re-words the provision on diacritic marks to take account of those marks whose presence or absence causes no change in spelling and applies the provision to names generally	this suggestion re-opens the decision taken at Copenhagen
inserts as a *Recommendation* the ethics that should guide an author re-naming a homonym	enacted as a mandatory Rule but with a proviso exempting the Commission from any responsibility in investigating or passing judgment on alleged contraventions
re-words the provision defining homonymy in regard to taxa belonging to categories in the Order/Class and Phylum-Groups, introducing the wording " objectively different "	
re-words the provision on family-group homonymy [on the lines of the provision above, item (153)] and adds an " Explanation " in the form of examples	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(155)	24/6(b)	Copenhagen : 37—Decision 55
(156)	24/7	Paris : 164—Conclusion 46(1)
(157)	24/8(a)	Copenhagen : 78—Decision 152
(158)	24/10(b	

Nature of difference from Congress Decision or from subsequent "Declaration"	Notes by "Règles" Section
adds example to the provision on the procedure to be followed in the case of family-group homonymy resulting from similar but not homonymous type genera	Compiler expresses the view in the discussion that in some cases the best solution of homonymy in family-group names resulting from similar type-genera might be to change the type-genera
adds footnote drawing attention to the force of the re-drafting of the Law of Homonymy in regard to generic names	
refers to exceptions to the Rule that generic names differing even only by a single letter are not homonyms	Compiler states that, although these were not expressly stated at Copenhagen as exceptions, they were intended as such [see item (150) above]
suggested statement on the Law of Homonymy in the case of specific names applying only to identical binomina	Compiler expresses the view that, although not expressly stated by a Congress, the enactment suggested is necessary for completeness

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(159)	24/10(d)	
(160)	24/11(b)(iii) 24/13(c)	Paris : 123—Conclusion 1(15)
(161)	24/11(d)	Copenhagen : 82—Decision 162
(162)	24/13(b)	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
new provision on a variant spelling or emendation of a generic name being disregarded in determining whether a specific name is a primary homonym	reference not understood [see items (79) (131) and (137)]
suggests the repeal of the provision on the status of a name proposed to replace a homonym or a supposed homonym when that homonym has not been validly rejected	Compiler expresses the view that such a name should have the status of a synonym
retains the provision on challenging the rejection of secondary homonyms but suggests a replacement which alters the procedure and omits the statement on the availability, after publication of the protest, of the replacement name and of the name of the senior secondary homonym	
" Explanation ", in the form of a hypothetical example, added to the provision on identical specific names in homonymous genera	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(163)	24/13(b)(i)	
(164)	24/15	Paris : 119— Conclusion 1(5)(6)(13)(14)
(165)	25/1	Berlin. Article 36 Paris : 118—Conclusion 1(1)
(166)	25/2	Berlin. Article 36

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
new provision against the reviving of a specific name rejected before the coming into force of the provision on identical specific names in homonymous genera	Compiler considers that although already covered by Draft 5/1 [item (10)] a definite statement should be made here
re-drafts considerably the Rules for the replacement of a specific name rejected as a homonym, condensing and amalgamating the points made at Paris	
retains statement on rejected homonyms in original Article 36 re-worded to incorporate Paris Conclusions	Article 36 was deleted altogether at Paris
retains the statement on rejected synonyms in original Article 36 and enlarges to incorporate Paris enactments	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(167)	26/	*Declaration* 4 Paris : 167—Conclusion 52
(168)	27/1(a)	Copenhagen : 47— Decision 78(1)(a)(i) : 52—Decision 86
(169)	28/1 footnote	
(170)	28/1(a)	Copenhagen : 55—Decision 93

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
retains nearly in full the original *Declaration* against using intemperate language, in the form of an " Explanation "	only the first sentence was incorporated in the *Code* at Paris, not the part in the " Explanation "
introduces, under the Rule of old Article 3 (requiring scientific names to be Latin or Latinised) the provision that an arbitrary combination of letters, latinised, comes under that Rule	the part on the arbitrary combination of letters was only mentioned in Copenhagen in connection with generic names (Article 8) and specific names (Article 14)
additional note on grammatical knowledge necessary for zoologists wishing to compile new names	
applies provision on the connecting vowels in compound names to all names (not only specific), adds a new interpretation of the above provision and refers to a new Table inserted at the end of the Article giving information on connective vowels	Compiler considers that, in fact, this provision was extended to all names by Copenhagen : 43, Decision 71 (1)(a)(i) [actually this provision applies only to generic and specific names]

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(171)	28/1 *Recommendations* 2, 3	Berlin. Article 20 First *Recommendation*
(172)	28/1 *Recommendation* 5	Berlin. Article 20 Third *Recommendation*
(173)	28/2(a)	Paris : 206—Conclusion 11(1)(d)
(174)	28/2(b)	Paris : 198—Conclusion 9(2)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
divides original *Recommendation* on only combining " *sub-* " and " *pseudo-* " with the appropriate language into two separate *Recommendations*	
suggests exceptions to the provision that proper names in languages not using the Latin alphabet should be transliterated according to the appropriate Schedule annexed to the *Code*	Compiler believes this to be a desirable addition as it allows the use of certain cases of names customarily published in a form which has not been transliterated according to the Schedule
broadens provision on specific names based on personal names ending in " q " to a general statement on all names	Compiler states that, although this was applied at Paris to specific names only, it should be broadened to include generic names
broadens provision, on specific names based on compound personal names and consisting of two words, to include the names of other taxa	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(175)	28/2 *Recommendation* 6	Paris : 208—Conclusion 11(5)
(176)	28/2 footnote	
(177)	28/2(c) (g) and *Recommendation*	Paris : 207—Conclusion 11(2)(c) Copenhagen : 55—Decision 95
(178)	28/2 *Recommendation* 9	Paris : 205—Conclusion 11(1)(a)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests broadening the *Recommendation* on specific names formed from compound surnames to cover all names	the Draft *Recommendation* would appear also to cover Berlin, Article 8 Second *Recommendation* (h)(ε) which is not inserted elsewhere
note on the formation of names of taxa from Spanish compound names added	
extends the provision on specific names, formed from two words of which the first denotes Christian Sainthood or is a nobiliar particle, to cover names for taxa belonging to any category ; extends the *Recommendation* (which applies, as at Copenhagen, only to specific names) to cover all types of " Saint " names	Compiler considers that there is confusion in the Copenhagen drafting in regard to Saint names and the draft of the postponed Paris proposal (which covered all Saint names) was more satisfactory ; [Copenhagen : 55, Decision 95 applies only to specific names and is cast in the form of a provision, although non-mandatory, and a *Recommendation* based on the provision but drafted with different wording as regards " Saint " names]
extends provision on specific names formed from persons of antiquity to cover the name of any taxon	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(179)	28/2(e)	
(180)	28/2 *Recommendation* 10	Paris : 205—Conclusion 11(1)(a)(ii)
(180a)		
(181)	28/2(f)(i)	Copenhagen : 48—Decision 80 : 55—Decision 92
(182)	28/2(f)(ii)	Paris : 206—Conclusion 11(1)(e) Copenhagen : 52—Decision 86(c)(iii)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
new provision on the treatment of proper names of certain nationalities when forming zoological names from them	Compiler states that this provision is suggested to cover certain cases brought to light by Dr. Sabrosky
widens application of the provision on names based on forenames of classical origin belonging to modern persons to include all taxa ; and adds sentence referring to the treatment of non-classical forenames	
	Compiler suggests that the *Recommendations* on generic and specific names based on modern surnames with prefixes should be revised and applied in like manner to each ; and that his provisions in Section 2(f)(h) applying to names in general should replace his tentative *Recommendation* (unnumbered following *Recommendation* 22 in Section 12) devoted to generic names only
applies the provision on generic and specific names based on modern surnames with " Mac" to names of taxa in general	see item (180)(a) above
suggests that the apostrophe should be omitted in names based on patronymics with " O' " and adds explanatory footnote on the meaning of " O' "	see item (186) below

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(183)	28/2(f)(iii)	
(184)	28/2(h)	
(185)	28/4(a)	Copenhagen : 57—Conclusion 101
(186)	28/4(b)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
new provision on names formed from Norman patronymics with the prefix " Fitz "	
drafts *Recommendations*, in expanded form and with new material, on the treatment of prefixes in personal names which are used as a basis for zoological names	these *Recommendations* are intended to supplant more limited *Recommendations* [see item (180)(a)] ; parts of them appear, however, to repeat general *Recommendations* already stated [compare *Recommendation* 12(α) with *Recommendation* 8, item (177)]
suggests that the use of a diaeresis should be an exception to the provision prohibiting diacritic marks	Compiler states that the diaeresis was in use in Latin ; a proposal on this subject is already before the Commission. B.**13** : 292
suggests new Rule that an apostrophe should not form part of a zoological name	see item (182) above

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(187)	28/5 *Recommendation* 13	Berlin. Article 19 *Recommendation*
(188)	28/5 *Recommendations* 15 and 16	
(189)	28/6	Copenhagen : 47— Decision 78(1)(a)(ii)
(190)	28/7	Berlin. Article 2 and Article 4
(191)	28/9(a)	Copenhagen : 34—Decision 50(1)(a)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests addition of specific mention that italics are usual in printing scientific names	Compiler considers that Copenhagen: 43—Decision 70 cancelling old Article 19 does not apply to this *Recommendation*
new *Recommendations* on the use of parentheses and brackets in connection with the names of taxa	
widens application of the provision on capital letters for generic names to apply also to the names of all taxa of higher category than genus	Compiler states that this is according to universal custom
states specifically that a family-group name should be a single noun in the nominative plural	no specific Congress statement as such, but implied in old Articles 2 and 4
adds examples to the provision on the formation of a family-group name from the name of a type genus which is of classical origin	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(192)	28/10	Copenhagen : 47—Decision 78
(193)	28/11(a)	Copenhagen : 49—Decision 84(1)
(194)	28/11(a)(i)	Copenhagen : 49—Decision 84 (2)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
alters the provision on the Rules for the formation of generic names omitting (a) the part on the single word being " either simple or compound " (b) the part " must be either a Latin word or a Latinised word " (c) the part that it must " consist of a single word " (d) the part on being treated as a noun in the nominative singular " by its original author " (e) the part on being written " with a capital initial letter " (f) the part on names prefixed with " Mac "	Compiler bases his draft not on the Official Record of the Copenhagen Congress but on the roneoed draft of the Copenhagen Colloquium report (Copenhagen MSS. 35, para. 20) as " The Copenhagen printed report . . . introduces more than one new element " the reasons given by the Compiler for omissions as enumerated opposite are as follows : (a) not a mandatory provision and so may be disregarded ; (b) and (c) are covered by old Articles 3 and 2 and are not therefore subject to automatic correction ; (d) the wording negates the idea of automatic correction ; (e) placed in a more general position covering genera and all higher taxa ; (f) not included in the MSS. Draft Report referred to above as a mandatory provision and should, in his view, only be a *Recommendation* [does not appear to be consistent with item (181) where it forms part of a general provision cast in mandatory form]
inserts in the provision on the gender of classical words being treated as given in lexicons the explicit statement " unless by contrary direction of the Commission " and adds an example and footnote explaining the method of indicating genders in lexicons	
substitutes the word " apparently " for " obviously " in the provision stating that names identical in spelling with Greek or Latin words are presumed to be taken from them	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(195)	28/11(b)	Copenhagen : 49—Decision 84(3)
(196)	28/11(d)	Copenhagen : 49—Decision 84(3)
(197)	28/11(d)(i)	Copenhagen : 50—Decision 84(7)(a)
(198)	28/11(d)(ii)	Copenhagen : 51—Decision 84(7)(b)

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
alters the provision on derivative classical nouns by omitting the word " nominative " before suffix and replacing the compound word examples given at Copenhagen with new examples ; adds grammatical explanations in footnotes ; places the examples of compound words given at Copenhagen in separate provision [see item (196) below]	Compiler expresses the view that the Copenhagen enactment is baffling and the examples given contrary to it
separate provision on possessive compound nouns or adjectives used as nouns	Compiler considers this provision necessary to fit the type of examples given in Copenhagen Decision 84(3)
uses " lexicon " instead of " dictionary " in the provision on Latin nouns to be treated as masculine names, and adds examples and footnotes	
re-arranges, re-words and adds new examples to the provisions on names to be treated as feminine	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(199)	28/11(d)(iii)	Copenhagen : 51—Decision 84(7)(c)
(200)	28/11(e)	
(201)	28/11(g)	
(202)	28/11 *Recommendation* 18	Copenhagen : 50—Decision 84(6)(d)

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
adds footnotes and examples to the provisions on names to be treated as neuter	
expands the draft of the suggested *Declaration* on the cases of the genders of certain generic names, which do not agree with the Copenhagen Rules and suggests further interpretations	based by Compiler on suggested *Declaration* B.11 : 259
new provision on the treatment of the gender of a generic name remaining in doubt under the provisions of the section in question	Compiler proposes to cover difficult cases [would not these cases be covered by *Recommendation* 18, item (202) below]
inserts as a *Recommendation* the provision on cases of doubt of the gender of compound words	Compiler interprets the Copenhagen decision as advisory and not as mandatory

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(203)	28/12 *Recommendation* 22	Berlin. Article 8 Second *Recommendation*(h)
(204)	28/12 *Recommendation* (unnumbered)	Copenhagen : 48—Decision 80
(205)	28/12(b)	Berlin. Article 8 Copenhagen : 47—Decision 78(2)
(206)	28/12(b)(3)	Paris : 264—Conclusion 37(2) Paris : 297—Conclusion 20

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
suggests widening the *Recommendation* on terminations for generic names based on modern surnames to apply also to those based on forenames	
inserts the provisions on names based on surnames prefixed with " Mac " as part of a tentative *Recommendation* in the Draft section on Generic Names	Compiler believes that the provision was not included as a mandatory portion of old Article 8 at the Colloquium [see item (192)], but states that this *Recommendation* should anyway be replaced by the proposed Draft sections 2(f) and (h) [see items (180)(a), (181)] which are cast in mandatory form to apply to names of taxa in general
re-drafts as mandatory provisions the section on the types of words which are available for generic names, adding new examples, footnotes and " Explanations "	these provisions appear at present in the *Règles* as *Recommendations*
adds " Explanation " on the principal types of Greek compound words with examples	

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(207)	28/13	Berlin. Article 14 Copenhagen : 51—Decision 86
(208)	28/16(b)(i)	Paris : 205—Conclusion 11(1)(b)(i) Copenhagen : 53—Decision 89
(209)	28/16(b)(i) (second sentence)	
(210)	28/16(b)(iv)(v)(vi)	Copenhagen : 53—Decision 89

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
alters, in the provision on the formation of specific names, the Copenhagen wording " may be " to " shall have "	the alteration in wording is to make clear that the provision is mandatory; retains in this provision the part on the need for the word being Latin or Latinised, but see Compiler's discussion of this clause in reference to generic names [point (b) of item (192) above]
re-words the part in the termination " -ii " in the provision on the formation of specific names) in the genitive singular) based upon modern surnames of men	Copenhagen Decision 89 reduced this Paris provision to apply only to surnames not Latin, Latinised or of Greek origin ; the Compiler considers that the wording used at Copenhagen in regard to the ending " -ii " is not correct as regards Latin words ; so he alters it throughout [see item (211) below and accordingly here, where, however, it appears to have a different meaning from the Copenhagen enactment
suggests new provision on a permissible treatment of specific names formed in the genitive singular from modern surnames of men which end in a short " a "	Compiler queries whether this provision should be included ; although noted as non-mandatory, it is not cast in the form of a *Recommendation*
inserts as three separate sub-paragraphs the provisions on the formation of specific names based upon modern patronymics of men in Latinised form	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(211)	28/16(b)(iv)	Copenhagen : 53—Decision 89 (first sentence)
(212)	28/16(b)(v)	Copenhagen : 54—Decision 89 (second sentence)
(213)	28/16(b)(vi)	Copenhagen : 54—Decision 89 (third sentence)

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
re-drafts the provision on specific names based upon modern patronymics of men consisting of Latinised words having the termination " -us " (a) altering to terminations in " -ius " and (b) re-wording the part on the genitive termination " -ii " ; adds examples	Compiler expresses the view (see Discussion in Draft, line 24, page 223) that the wording of Copenhagen Decision 89 " is not quite correct as regards Latin and Latinised words " ; so alters as in (a) opposite because he states that a modern surname is Latinised by adding the termination " -ius " and re-drafts as in point (b) because he believes the wording used at Copenhagen to be wrong as "there is no such thing in Latin as ' genitive case-ending -ii ' " [the expression used in the Copenhagen Decision is " genitive termination (singular, ' i '—or, less desirably, ' -ii ') "] ; the example given on Linnaeus does not appear to be appropriate here and this example is also inserted later in *Recommendation* 25a of the same section [see item (219) below]
suggests altering " having the termination ' -a ' " to " has any termination other than ' -ius ' " in the provision on names based on modern patronymics of men Latinised and terminating as above	
gives new version to replace the provision on the formation of specific names based on modern patronymics of men with the termination " -us " or " -a " but of Greek not Latin origin making it necessary first to follow, if possible, grammatical usage as indicated in lexicons	Compiler believes that the Copenhagen enactment " is improper from the viewpoint of Latin " and alters accordingly

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(214)	28/16(c)(i)	Paris : 205—Conclusion 11(1)(b)
(215)	28/16(c)(ii)(iii)(v)	Copenhagen : 54—Decision 90
(216)	28/16(c)(iii)	Copenhagen : 54—Decision 90 (second sentence)
(217)	28/16(c)(iv)	

Nature of difference from Congress Decision or from subsequent “ Declaration ”	Notes by “ Règles ” Section
applies to names in general, not only to surnames, and adds a clause making it permissible to drop the final vowel of the person’s name in the provision on the formation of specific names based upon modern surnames of women	Copenhagen : 54, Decision 90 deals with such names as are Latinised or of Greek origin, thus excluding such cases from this Paris provision
inserts as three separate sub-paragraphs the provisions on the formation of specific names based upon modern patronymics of women in Latinised form and applies to names in general not only to patronymics	
gives new version of the provision on specific names based upon modern patronymics of women Latinised and ending in “ -us ” replacing the part which provides that the case-ending is to be added to the entire word	Compiler suggests as better Latin
new provision on specific names formed from the names of modern women Latinised with a termination other than “ -a ” or “ -us ”	Compiler believes to be necessary for completeness [corresponding to item (212) above]

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(218)	28/16(c)(v)	Copenhagen : 54—Decision 90 (third sentence)
(219)	28/16(d) *Recommendation* 25a	Paris : 206—Conclusion 11(1)(c)
(220)[1]	28/16(f)	Copenhagen : 53—Decision 87
(221)	28/17 *Recommendation* 26 and footnote	Copenhagen : 57—Decision 99

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[1] See also Item (250) which should have come between Items (219) and (220).

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
gives a replacement for the provision on specific names based on modern patronymics of women with the ending " -us " or " -a " when a word of Greek not Latin origin ; the new version modifies the Rule that the termination is to be added to the entire word	Compiler expresses the view that the Copenhagen provision results in impossible Latin in the cases in which a double " a " would be produced
inserts as a *Recommendation* the provision on the formation of specific names from the surnames of certain persons (Linnaeus, etc.)	although reduced to a *Recommendation* by Copenhagen : 53, Decision 86(2) this treatment is not consistent with the use of the examples of Linnaeus and Fabricius in the Draft mandatory provision above [item (211)]
re-words the provision on contraventions of (old) Article 14 being " subject to automatic correction " to "must be corrected when noticed"	this wording does not appear quite to make clear that in the case of such corrections original authorship and date are to be retained [see also Draft 6/5(f) where this section is not mentioned]
suggest two replacements, for the *Recommendation* on the formation in adjectival form of specific names based on geographical terms by adding the termination " -ensis ", (a) in the form of a re-drafted footnote for such geographical terms as are Latin words and (b) in the case of such terms as are barbarous place-names in the form of a *Recommendation* modified to allow for the final vowel of the place-name to be retained in certain cases	Compiler holds the view that Copenhagen Decision 99 " is grammatically incorrect " and that as far as Latin words are concerned it is sufficient to insert a footnote, re-worded to result in correct Latin and covering endings in general (as he considers that this part is a statement of fact) and to leave the *Recommendation* to deal with barbarous place-names

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(222)	28/18(d)	
(223)	28/18(f)	Berlin.—Article 15 Second paragraph
(223)(a)		
(224)	29/1	
(225)	29/3(b)(v)	Paris : 40—Conclusion 1(2)(e)

Nature of difference from Congress Decision or from subsequent “ Declaration ”	**Notes by “ Règles ” Section**
new provision on certain cases of words compounded with a capital letter being treated as compounded with a sign	
gives further example in the provision on the rejection as names of two or more words related by conjunctions adds new Tables as an Appendix to the Article to guide taxonomists in formation of names from Latin and Greek	
adds statement defining the status of the Commission	
suggests enlarging the provision on scientific representation among the Commissioners by adding the phrase “ other fields of biology including ”	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(226)	29/3(g)(i)	Copenhagen : 89—Decision 174(2)
(227)	29/3(g)(ii)	Copenhagen : 89—Decision 174(3)
(228)	29/3(j)	
(229)	29/4	Paris : 325—Conclusion 15(c)

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
substitutes the word " organisation " for " institution " in the provision on interim elections of Commissioners	Compiler considers that the word " institution " is too limited in meaning and suggests that it would be better to announce, in the *Bulletin*, the names of organisations to be consulted
	Compiler queries the meaning of the procedure prescribed in cases where only unsuitable candidates are put forward as proposed Commissioners during an inter-Congress period
new provisions on the duties of Commissioners	
suggests adding sentence defining the duties of Officers of the Commission	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(230)	29/4(a)	
(231)	29/4(c)	Paris : 326—Conclusion 15(c)(d)
(232)	29/6(c)	*Declaration* 5 Paris : 56—Conclusion 7(2) : 324—Conclusion 14
(233)	29/6(c)(v)	Berlin. Plenary Powers Resolution, third paragraph (added at Monaco, 1913)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
adds sentence limiting those eligible for Office to Commissioners	suggested by Compiler as a new Rule
suggests additions further defining the procedure by which, in inter-Congress periods, vacancies in offices are to be filled by the Commission	
re-drafts explaining the type of action that may be taken under Plenary Powers and specifying certain instances ; adds that the Powers are subject to certain provisions stated in the section	provisions to which the exercise of the the Plenary Powers are subject are on the voting procedure to be followed in such cases and the principles to be followed in the suppression of names [see also item (11)]
restores the part on the Powers of the Commission being especially suitable for application in the case of the names of larval stages	this part was deleted by Copenhagen: 23, Decision 20(2)

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(234)	29/6(c)(vi)	Paris : 339—Conclusion 23(1)
(235)	29/7	Paris : 292—Conclusion 10(a)(4)
(236)	29/7(b)	
(237)	29/7(s)	

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
adds qualifying phrase to the part on the Commission being guided by certain principles in exercising its Powers in regard to the suppression of names	Compiler suggests the addition to allow for more flexibility of practice
defines an *Opinion* and elaborates the statement on the duties of the Commission in regard to *Opinions*	
inserts definite statement on the duties of the Commission in regard to suspected non-binominal works	the duties here assigned to the Commission have been accepted by that body and acted on in a number of cases, but there does not appear to be any explicit Congress Decision on this point
new provision on the duty of the Commission in regard to authorising the selection of neotypes from other material if the existing type material is faulty	Compiler proposes to insert if the previous provision on faulty existing type material is accepted [see items (112) (118) above]

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
(238)	29/9(c)	
(239)	29/9(d)	
(240)	29/9(d)(i)	
(241)	29/9(e)	Paris : 267—Conclusion 41 : 270—Conclusion 42 Copenhagen : 37—Decision 58 : 38—Decision 62 *Declaration* 18 *Declaration* 19

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
inserts definition of a *Direction*	
inserts paragraph on the duty of the Commission in regard to the *Bulletin* and defines the aims of the *Bulletin*	
lists notices which, according to various Rules, are to be published in the *Bulletin*	
omits details of Rules applying to entries on *Official Lists* and *Indexes*	under existing Congress Decisions the particulars here omitted were to be inserted in the *Règles*

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(242)	29/10(a)(b)(c)	
(243)	29/11	
(244)	29/12(a)(1)	Paris : 50—Conclusion 6(a)
(245)	29/12(a)(3)(i)	

Nature of difference from Congress Decision or from subsequent " Declaration "	Notes by " Règles " Section
inserts provisions, not previously formulated, on action that is not the duty of the Commission	Compiler suggests that these novel provisions would serve to prevent misapprehensions
inserts new provision on sessions of the Commission and the transaction of business during the intervals	added by Compiler for completeness
alters the wording, in the provision on the procedure for the adoption of an *Opinion* at a meeting, to make the necessary majority calculable on all those Commissioners or alternates present not on all Commissioners	Compiler states that the provision as drafted at Paris would lead to a procedure not intended or followed
inserts new sentence defining " all Commissioners " as the term is used in regard to voting procedure [apart from that specified in item (244) above] in connection with *Opinions*	

Item No.	Reference to Draft of " Règles "	Reference to Congress Decision or to subsequent " Declaration "
(246)	Glossary	
(247)	Glossary/date	Paris : 223—Conclusion 18(a)
(248)	Glossary/homonym	Paris : 118—Conclusion 1(2) : 344—Conclusion 28
(249)	Glossary/infra-subspecific form	Paris : 90—Conclusion 1(3)

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
definitions of expressions employed are grouped at the end of the Draft in a list entitled " Glossary "	no provision for a " Glossary " has been made by the Congresses which decided that definitions of expressions should be included in the *Règles* at the appropriate points ; the Draft contains no Article defining the status of the " Glossary "
adds to the definition of " date " that it must be reckoned according to the Gregorian Calendar and omits " whole " which qualified " edition " in the part on distribution free of charge	
re-words the definitions of primary and secondary homonyms	Compiler considers that, as enacted at Paris, the definitions are not sufficiently broad, and do not cover subspecies properly
re-words and narrows scope of the definition of an infra-subspecific form	Compiler notes that, according to Dr. Sabrosky, the names of aberrations which are teratological specimens are excluded from zoological nomenclature and should not therefore, be included in the definition

Item No.	**Reference to Draft of " Règles "**	**Reference to Congress Decision or to subsequent " Declaration "**
Additional Item (250)	28/16(e)	Paris : 251—Conclusion 13

Nature of difference from Congress Decision or from subsequent " Declaration "	**Notes by " Règles " Section**
omits, in connection with the provision on modern surnames used in an unchanged form for specific names, the definite statement that the provision that a specific name may be a noun in the nominative singular in opposition to the generic name, does not apply in the case of a specific name based on a modern surname	

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- 6 MAR 1958

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THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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* Professor Esaki died on 14th December 1957, while the present Part was passing through the Press.

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Volume 15, Double Part 16/17 (pp. 489—556) 21*st March* 1958

CASE No. 25

DRAFT "REGLES", ARTICLE 12, SECTION 1 (NAMES FOR TAXA OF THE ORDER/CLASS AND HIGHER CATEGORIES)

(Commission Reference : Z.N.(S.) 1242)

(For the relevant provision in the Draft *Règles* see 1957, *Bull. zool. Nomencl.* 14 : 92)

PURCHASED
31 MAR 1958

DOCUMENT 25/1

Problems involved in the stabilisation of the names for Orders, Classes and Taxa of Higher Rank

By **FRANCIS HEMMING,** C.M.G., C.B.E.

(*Secretary to the International Commission on Zoological Nomenclature*)

(Note dated 1st January 1958)

The present note, which is in the nature of an Interim Report on certain problems arising in connection with the plan for stabilising the names for Orders, Classes and Taxa of Higher Rank adopted by the Copenhagen Congress in 1953 (1953, *Copenhagen Decisions zool. Nomencl.* : 38–43, Decisions 59–69), has been prepared in the hope that it may prove of assistance to the Colloquium to be held in London next July, if, after it has completed the task of examining the remaining portions of the Draft of the *Règles* for submission to the Fifteenth International Congress of Zoology for final approval, it finds it possible to carry the consideration of this important matter forward to a further stage.

2. The scheme agreed upon by the Copenhagen Congress, it will be recalled, had two basic features, namely (1) that the names for at least the more important taxa of the Order/Class and Higher Categories should be stabilised by being placed on an *Official List* then established for the purpose and (2) that the nominal taxa belonging to the foregoing categories should be given a determinate content by being provided with type genera to be selected in harmony with current usage. No agreement was reached as to how the nominal taxa to be stabilised in this way should be selected, and in consequence all that it was possible to agree upon was that committees of specialists should be established to prepare and submit for eventual approval lists of recommended nominal taxa, with type genera, for the various groups in the Animal Kingdom. It was further decided that in preparing the suggested lists the Committees should " give first consideration to weight of usage, and, where usage affords no clear basis for choice, to other considerations, such as priority, the nature of such considerations to be specified in the list to be prepared " (Decision 62(1)).

3. At this point I must report that, as Secretary to the Commission, I initiated certain consultations subsequent to the close of the Copenhagen Congress which led me to the view that at least until the ground had been more thoroughly prepared by individual specialists in the various groups, committees, if established, could not hope to achieve any fruitful results. For it very soon became evident that the number of names to be considered was very much larger than had been anticipated by the Copenhagen Congress and the problems involved far more intricate than had then been supposed.

4. A pre-requisite to any scheme for providing stability for the principal Order/Class names of any group must be a reasonably complete and accurate knowledge of what are the names involved, so that not only may a choice be made of those names which it is desired should be included in the *Official List* for protection but also that appropriate arrangements may be made for preventing names which are subjective senior synonyms of the names to be stabilised from entering into competition with those names and thus leading to further lack of uniformity in the nomenclature for taxa of these categories. It is here that we encounter a major difficulty which, except in the case of a few exceptionally well-placed groups (paragraph 5 below), would render impossible the task of any *ad hoc* committee which might be appointed. I refer to the total lack at present of any work containing an enumeration of the names so far published for Orders (including Sub-Orders) and taxa of higher rank comparable in scope to the great *Index Animalium* compiled by the late Charles Davies Sherborn. A similar, though perhaps rather less serious, difficulty confronts any zoologist who may have occasion to ascertain what are the oldest available family-group names in his speciality. In this connection I must refer to a scheme for the preparation of a work containing bibliographical references for all names published for taxa belonging to the

Family-Group, Order/Class Group and Higher Categories which I have already submitted and which appears on the London Agenda Paper as Document 5/1.

5. Pending the preparation of the work described above, it cannot, I think, reasonably be expected that it will be possible to secure any general advance in the matter of stabilising the names for Orders and Classes all along the front from the Protozoa to the highest groups in the Urochorda. This does not mean, however, that no immediate progress is possible in any part of this field. But it does mean that such advance is only to be expected at those points where by reason of undertaking large-scale revisions specialists in particular groups find it necessary for the purposes of their own work themselves to make a detailed survey of the literature for the purpose of drawing up lists of the Order/Class Names which it is necessary for them to consider. In two cases investigations by individual specialists have been undertaken since the Copenhagen Congress and the results communicated to the Office of the Commission. The first of these is a survey of the names involved in the Class Echinoidea undertaken jointly by R. V. Melville (*Geological Survey and Museum, London*) and J. Wyatt Durham (*University of California, Berkeley, California, U.S.A.*). The second, of which an abstract only has so far been received, is concerned with Order/Class Names in the Sub-Phylum Ciliophora and has been prepared by Professor John O. Corliss (*Department of Zoology, University of Illinois, Urbana, Illinois, U.S.A.*). The material assembled by these specialists for the above groups concerned, together with their suggestions as to the genera to be selected as type genera, provide the basic material needed for the preparation of definite proposals for final approval. It is not suggested that the London Congress should take either of these cases into immediate consideration but it is thought that it would be helpful, as showing the large amount of preliminary investigations needed in cases falling in this field, if the Congress were to have before them the two papers referred to above. The paper on the Class Echinoidea by Melville and Durham is accordingly being placed on the London Agenda Paper as a paper submitted for information only and has been allotted for this purpose the Document Number 25/2. The abstract in regard to the Order/Class names in the Ciliophora submitted by Professor Corliss is being placed on the Agenda Paper on the same basis as Document 25/3.

6. The difficulties involved in making progress in this field are not confined to those arising from the lack in the great majority of cases of sufficient information as to the names to be considered and their relative dates. For there is at least one major question on whch opinion is divided among those specialists who have communicated with the Office of the Commission. This is the question of the role which should be allotted to the principle of priority

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[1] See pp. 187–193 of the present volume.

in determining the status to be accorded to names belonging to the Order/Class and Higher Categories. As regards this, a wide variety of views has been expressed. These may be summarised as follows :—

(a) In favour of priority being accepted as the means for determining the relative status of names of the Order/Class, etc. Groups :—

Melville & Durham (Document 25/2)

Corliss (Document 25/3) who recognises that the Plenary Powers will need to be used to protect well-known names in certain cases

Dougherty (E.C.) (Document 25/4)

Brown (D.A.) (Document 25/5)

(b) In favour of usage being treated as of being of greater importance than priority :—

Cox (L.R.) (Document 25/6)

Hopkins (G.H.E.) (Document 25/7)

Chitwood (B.J.) (as is made clear by E. C. Dougherty's correspondence with that specialist reproduced in Document 25/4)

(c) Opposed to any form of regulation which includes the designation of type genera for nominal taxa of the Order/Class, etc. Groups :—

Lemche (H.) (Document 25/8) ;

(d) In favour of usage prevailing over priority until some date to be specified by the Congress and thereafter priority to prevail :—

Bradley (J.C.) (in draft of Revised *Règles*, 1957, *Bull. zool. Nomencl.* **14** : 92).

7. Allied to some extent with the problem discussed above is that presented by cases where there is an absolutely straight choice between two-well known names for a single taxon, both of which are, and/or have been, extensively used. Shall priority prevail in such a case or would it be better for an arbitrary choice to be made ? This problem is illustrated by the case of the names POLYZOA and BRYOZOA discussed by Brown in Document 25/5 where the adoption of the principle of priority is strongly argued. In the next paper, Document 25/6, the problem presented by the name to be used for another Class of invertebrates is discussed by Dr. L. R. Cox who is opposed to the application of the priority principle for determining the status of names for taxa at the Order/Class level. This case is of interest also as illustrating another

problem which arises sometimes at the Order/Class level, namely the existence of homonymous names for taxa in different parts of the Animal Kingdom. In this instance the name concerned is Loricata which has not only been applied to the Class of the Mollusca with which Dr. Cox is concerned but has been employed also for nominal Order/Class taxa both in the Class Reptilia and in the Class Mammalia. Another situation which may arise is illustrated by the problem presented by the question of the name to be used for the Order of insects comprising the Fleas discussed by Hopkins in a paper originally prepared as part of a questionnaire to flea-specialists on the question of name to be accepted for the Order (Document 25/7). Here we have a clearly defined and well recognised Order for which a number of names have been published but none has acquired such general and predominant usage as to secure for itself an unquestionable claim for acceptance at the expense of the other names in use by some specialists.

8. If in cases of the kind discussed above the priority principle were to be applied, a definite answer would automatically be obtainable, but it could not reasonably be expected that the answer so obtained would necessarily be the answer desired by specialists and the one which would best promote stability and universality in nomenclature. If on the other hand, the " weight of usage " principle were to be applied, it could hardly be hoped that committees of specialists of mixed composition could succeed in suggesting satisfactory solutions. To take, for instance, the case of the Ordinal name to be used for the Fleas, a committee of entomologists, some of whom were specialists in other Orders, would be unlikely to command sufficient authority among flea-specialists to secure general support for its findings, even if the members of such a committee were willing to make a recommendation of this kind on a subject outside their own fields. On the other hand, it will be generally agreed that, if the " weight-of-usage " principle were to be adopted, a thorough canvass of opinion would be needed among the specialists directly concerned. Consultations so undertaken would secure all the advantages which could be obtained from a formally constituted committee and by their greater flexibility and greater homogeneity would, in my view, be much to be preferred. Indeed, this method of procedure would seem the only one practicable in cases where—as in the case of the Class Name for the genus *Chiton* raised by Dr. Cox in Document 25/6—identical names for two or more Order/Class taxa in different parts of the Animal Kingdom are in competition with one another, for in such a situation separate consultations would need to be undertaken with specialists in each of the groups concerned. Consultations so undertaken would throw valuable light on the importance from the " usage " standpoint of the names concerned in each of the groups involved but could not be expected to provide an agreed recommendation supported by specialists in all of those groups, save in the most exceptional circumstances. Clearly, in such cases, the Commission alone—in virtue of its judicial function—would possess the authority requisite for promulgating a decision that would be generally—even if, in some cases, regretfully—accepted by all concerned.

9. The papers submitted raise other issues on which decisions are called for :—

(a) Are vernacular names to be excluded from account if the priority principle is adopted (Document 25/2, paragraph 2) ?

(b) Are names which, when first published, appeared as more than one word to be excluded (Document 25/2, paragraph 2) ?

(c) What Rule should be applied to the naming of nominate sub-taxa of the Order/Class and Higher Categories (Document 25/2, paragraph 3) ?

(d) Should the rules provide that only a genus which is itself the type genus of a family is eligible for designation as the type genus of a taxon belonging to the Order/Class Category (Document 25/2, paragraph 3) ?

(e) Should the selection of a type genus for a taxon belonging to a given series in the Order/Class Group constitute automatically a selection of the same taxon as the type genus of every taxon of lower rank in the same series within the Order/Class Category (as in the parallel case of type selections for taxa belonging to the family-group category) (Document 25/2, paragraph 3) ?

(f) When a name currently used as the name of a taxon of the Order/Class Group was first published as the name for a taxon of the Family-Group Category, should that name, as used for the former, rank as from the first time that it was so used or should it rank from the earlier date on which it was first used as the name for a family-group taxon (Document 25/2, paragraph 4) ?

(g) For the purposes of the Law of Homonymy should the termination used for an Order/Class Name be disregarded (Document 25/2, paragraph 5 ; Document 25/4, Appendix III) ?

(h) Would it be a good plan to insert in the *Règles* a *Recommandation* that, where within a given major taxon in the Order/Class Group names are in general formed in accordance with a single principle, authors should guide themselves by that principle when publishing names for new taxa within that major group (Document 25/2, paragraph 6) ?

(i) When selections of type genera for nominal Order/Class taxa are being made, can a genus that is already the type genus of one such taxon be selected to be the type genus of another such taxon, in order permanently to invalidate the later-published name (Document 25/2, paragraph 7) ?

(j) What should be done with senior subjective synonyms ? Should they be suppressed under the Plenary Powers ? (Document 25/2, paragraph 8) ?

(k) Is the field of choice for the selection of type genera for nominal Order/Class taxa limited to genera expressly cited at the time of the establishment of the Order/Class taxon concerned (Document 25/2, paragraph 9) ?

(l) In order to secure the status of availability for an Order/Class name must some " indication " be given at the time of its first publication (see the note by Knight (J.B.), Lemche (H.) and Yochelson (E.L.), reproduced as Document 25/9) ?

10. It is clearly desirable that the London Congress should provide definite answers to the questions listed in the preceding paragraph and to any others of a similar character which may come to light. In addition, it will be desirable that further consideration should be given to the question of the procedure to be adopted for formulating, and for dealing with, applications for the stabilisation of names for taxa belonging to the Order/Class and Higher Categories. As regards this, it is suggested for consideration that the best course would be to include in the *Règles* provisions on the lines set out below. In Alternative " A " this suggestion is outlined on the assumption that, as agreed upon at Copenhagen, usage rather than priority is to be the main factor in determining what names are to be accepted for taxa of the Order/Class and Higher Categories. In Alternative " B " particulars are given as to the modifications which would be needed if priority were to be accepted as the guiding principle (subject to the use of the Plenary Powers where necessary to prevent the overturning of well-known names). The suggestions now submitted could be combined if, as suggested by Professor Chester Bradley, the " usage " principle were to be adopted for names published before a certain date and the " priority " principle after that date.

Alternative " A " : Procedure suggested if the " weight of usage " principle is accepted as the determining factor for the acceptance of names for nominal taxa of the Order/Class and Higher Categories

(1) Where in the case of any substantially self-contained taxon of the Order/Class Group or Groups of Higher Rank, specialists assemble sufficiently complete data as to the names already published for that taxon and for taxa of lower rank comprised therein, the paper so prepared shall be submitted to the Commission, whose duty it shall be to arrange for its publication in the *Bulletin of Zoological Nomenclature*, so that it may serve as a basis for further discussion.

(2) In papers published under (1) above type genera shall be specified for each of the nominal taxa of Ordinal or higher rank concerned but type selections so made are to be treated as being provisional in status only and shall not acquire any status under the Law of Priority in virtue of being so published.

(3) Public notice of the publication of papers dealing with the above questions shall be issued by the Trust in the same way as such Notices are required to be issued in connection with applications for the use of the Plenary Powers in particular cases.

(4) After the expiry of such period, not being less than six calendar months, as the Commission may in any case consider necessary, it shall be the duty of the Commission to issue directions as to which of the names concerned are to be accepted as being available names and which are to be rejected as being unavailable.

(5) Subject to due compliance with the foregoing procedure the Commission shall not be required to use its Plenary Powers for the purpose of rejecting a name of the Order/Class and Higher Categories when that name is a senior subjective synonym of a name which it may decide to preserve.

(6) When under (4) above the Commission issues a direction either accepting as available, or rejecting, a name for a taxon belonging to the Order/Class or Higher Categories, it shall be its duty to place that name on the *Official List* or, as the case may be, on the *Official Index of Rejected and Invalid Names*, the entry so to be made to specify in each case the type genus of the nominal taxon concerned.

Alternative " B " : Procedure suggested if the " priority " principle is accepted as the determining factor for the acceptance of names for nominal taxa of the Order/Class and Higher Categories

As in " A " above, except that (5) would no longer be applicable.

DOCUMENT 25/2

Questions relating to Order/Class Group nomenclature in the Class Echinoidea

By **R. V. MELVILLE**

(*Geological Survey and Museum, London**)

and

J. WYATT DURHAM

(*University of California, Berkeley, California, U.S.A*).

(Enclosure to a letter dated 4th January 1957)[1]

Editorial Note : What was substantially the same paper as that here reproduced was published by the present authors under the title " A Classification of Echinoids " in January 1957 (*J. Palaeont.* **31** (No. 1) : 242–272). The paper as here reproduced differs from that already published by the authors in that they have (a) corrected one erroneous date (that for Echinideae Claus), (b) added seventeen names previously omitted and (c) have changed the treatment accorded to twenty other names. Particulars of the changes so made were communicated to the Office of the Commission by Mr. Melville on behalf of his co-author and himself in a letter dated 20th November 1957. The revisions so made by the authors affect their conclusions as set out on pages 267–270 of the paper referred to only in that the Order name Stereosomata Duncan, 1889, is now made a junior objective synonym of Echinideae Claus, 1880, and is not regarded as an available name. (Intl'd 23rd January 1958. F.H.)

In considering the names to be used in the *Treatise of Invertebrate Paleontology* for taxa in the Order/Class-group in the Echinoidea, the writers compiled as complete a list as possible of the relevant names. These lists were then considered in the light of the decisions of the Fourteenth International Congress of Zoology, Copenhagen, 1953, relating to names in the Order/Class-group (*Copenhagen Decisions zool. Nomencl.*, 1953, Decisions 59–69, pp. 38–43).

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* By permission of the Director, Geological Survey and Museum.

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[1] This case has an associated Registered Number Z.N.(S.) 1194.

The Decisions referred to envisaged the setting-up of committees of specialists in each group to consider Order/Class-group nomenclature. Since no committee is yet in being to review the subject so far as the Class Echinoidea is concerned, it seems best to us to publish the lists that we have compiled in the *Bulletin of Zoological Nomenclature*, so that other specialists can criticize our conclusions and supplement our lists prior to the establishment of an *Official List of Names in the Order/Class-group* in Echinoidea. We are grateful to the Secretary of the International Commission on Zoological Nomenclature for his support and for the opportunity he gives us of getting our difficulties more widely discussed. We therefore state the terms of reference that we have adopted and set forth the problems that we have met, and append the lists of names.

2. Our lists exclude all names that have been proposed in the vernacular and all those consisting of more than one word, even if correctly latinized. They include only latinized names referred to recognized categories (Suborder, Order, Superorder, Subclass, Class) and latinized names not formally categorized but readily recognizable as equivalent to names in the recognised categories, as well as names qualified by unaccepted terms such as " Grade " or " Tribe ".

3. All names in the Order/Class-group have been treated as nomenclatorially co-ordinate to a limited extent ; that is, a name introduced for a taxon at one level in the group is regarded as available with its original authorship and priority at all other levels in the group (with appropriate change of termination). We find, however, that to adopt the corollary of nominate sub-taxa would lead to the loss of well-established names. We have assumed that a nominal genus selected as type-genus of a nominal taxon at any level in the Order/Class-group must *ipso facto* be type-genus of a taxon at every lower level in the group, as well as of taxa at every level in the Family-group. This procedure seems to us a logical extension of Article 29.

4. Some familiar Order/Class-group names in the Echinoidea were first proposed in the Family-group, but have since become attached to taxa in the Order/Class-group. For instance, the names Regularia and Irregularia were first used by Latreille, 1825, as Subfamily-names ; they were first applied to taxa in the Order/Class-group by Carus (1863) and have been used by later authors (including Mortensen, 1935) as Subclass-names with attribution to Latreille. Again, the familiar Order-name Clypeasteroida has generally been attributed to L. Agassiz, 1836, although his original name was in the vernacular (" les Clypéastres ") without any category-name. We give both these sorts of names priority only from their first usage in latinized form for taxa in the Order/Class-group. Historically, the Phylum Echinodermata was for long regarded as a Class (by one author until at least 1857) and the Class Echinoidea as an Order, so that names in the Order/Class-group are generally of later origin than names in the Family-group as such.

5. We have extended Article 34 (the Law of Homonymy) in a strict sense in that we have considered only the first use of each word ; but we have not regarded a difference in termination as creating a condition of homonymy. In this we disagree with *Copenhagen Decisions* paragraph 68(1) because we feel that it is better to use the terminations now generally standardized in the Echinoidea and to dispose of similar words with different terminations by means of objective synonymy. Only identical words have been treated as homonyms.

6. The majority of Order/Class-group names in the Echinoidea have been formed from the name of an included genus, partly because many of them were first proposed as Family-names. This fact has an obvious influence on the selection of the type-genus. There are also names formed from the names of what we consider marginal genera or *nomina dubia* (see *Copenhagen Decisions* para. 62(3)(*a*) ; names formed from generic names modified by the addition of a prefix ; and names formed from the name of a morphological character thought to indicate relationship between the forms endowed with it. Names of this last kind are relatively few and have generally been short-lived in practice. It will probably be found that in each major taxon, Order/Class-group names will tend more and more to have been formed in accordance with a single principle—in the Phylum Arthropoda, for instance, names formed from the names of morphological characters are obviously prevalent in current usage. We suggest that the International Commission might recommend that, in future, new Order/Class-group names should be formed in accordance with the principle prevailing in the taxon in question. We regard this not as a restriction of taxonomic freedom, but as an extension of the power of control over the mode of formation of names in the Family-group already embodied in Article 4.

7. Following *Copenhagen Decisions* paragraph 62(4) we have tried to provide an available name for every currently-recognised taxon. We seek clarification of the question whether this paragraph means that every accepted nominal taxon must have a different type-genus. If two mutually exclusive nominal taxa exist at the same level, we see no objection to their having the same type-genus. For example, the Class Echinoidea is currently divided into two Subclasses in two different ways. Mortensen (1935) recognizes the Subclasses Regularia and Irregularia ; Durham and Melville (1957) recognize the Subclasses Perischoechinoidea and Euechinoidea. These four names, with type-genera as chosen by us, are :—

Perischoechinoidea McCoy, 1849—*Palaechinus* McCoy, 1844
Euechinoidea Bronn, 1860—*Echinus* Linnaeus, 1758
Regularia Carus, 1863—*Echinus* Linnaeus, 1758
Irregularia Carus, 1863*—*Spatangus* Leske, 1778

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[*] It will be seen from the lists of names below that we regard the names Regularia and Irregularia as objective junior synonyms of Endocyclica Bronn, 1860 and Exocyclica Bronn, 1860, respectively.

We hope that our selection of the nominal genus *Echinus* Linnaeus, 1758, as type-genus both of Euechinoidea Bronn, 1860 and of Regularia Carus, 1863 will meet with the approval of our colleagues. Since it is inconceivable that any taxonomic scheme would use both Euechinoidea and Regularia as Subclass-names, we see no objection to a state of objective synonymy existing between them. Any scheme that used both names would apply them to taxa at different levels. This objective synonymy is in effect inevitable if we have correctly applied the meaning of Article 29 in Paragraph 3 above.

8. It is not clear from *Copenhagen Decisions* paragraph 62(3)(*b*) what is to be done with unwanted names that become objective senior synonyms of names that are to be added to the *Official Lists*. We favour, as the least equivocal course, the use of the Plenary Powers to suppress these names.

9. We have extended Article 30(e)(α) in assuming that the type-genus of a nominal taxon in the Order/Class-group must be selected from among the genera explicitly included in the taxon by the author of the name at the time when he first proposed it. If this is rigidly construed, then two names in the Echinoidea will have to be preserved although they serve no useful taxonomic purpose and were from the beginning subjective junior synonyms of existing names. The nominal Order Nodostomata Lambert, 1915 has as type by monotypy the nominal genus *Amblypygus* L. Agassiz, 1840 (the only nominal genus mentioned in connection with the name Nodostomata when this was first proposed). *Amblypygus* is subjectively a member of the Order Cassiduloida Claus, 1880 (as interpreted by Mortensen, 1948). The name Nodostomata is proposed by Lambert in a more or less casual manner in a discussion of the differences between *Amblypygus* and *Echinoneus* Leske, 1778, which is subjectively a member of the Order Holectypoida Duncan, 1889, but for which Lambert (1915) proposed the Suborder-name Globatoroida in a similarly casual manner. When next used (Lambert, 1918), the nominal Order Nodostomata included two Suborders, Procassiduloida Lambert, 1918 and Spatangoida; the former contains, among others, the nominal genera *Amblypygus* and *Procassidulus* Lambert and Thiéry, 1918. This latter name was proposed as a substitute name for *Cassidulus* Lamarck, 1801, which was held to be invalid as a homonym of *Cassidula* Humphrey, 1797. Apart from the fact that there is not a true condition of homonymy in this case, the work in which the name *Cassidula* was published (the anonymous *Museum Calonnianum*) was ruled as not available for nomenclatorial purposes in *Opinion* 51. The type-species of *Cassidulus* is *C. cariboearum* Lamarck, 1801, and under Article 30(f) and *Declaration* 27 this must also be the type-species of *Procassidulus*, in spite of the original designation of *Echinites lapiscancri* Leske, 1778. The most satisfactory course would be to use the Plenary Powers to designate the nominal genus *Cassidulus* Lamarck, 1801, as type-genus both of the nominal Suborder Procassiduloida Lambert, 1918, and of the nominal Order Nodostomata Lambert, 1915; if our selection of *Cassidulus* as type-genus of the nominal Order Cassiduloida is upheld, then both Procassiduloida and

Nodostomata will become objective junior synonyms of Cassiduloida. Another generic name will then have to be found for the group of species of which *Echinites lapiscancri* Leske, 1778, is one. Again in 1918, the nominal Suborder Globatoroida includes the nominal genus *Globator* L. Agassiz, 1840, which was presumably the source of the Order-group name. *Globator* is regarded by many specialists as a *nomen dubium*, possibly a subjective junior synonym of the nominal genus *Pyrina* Desmoulins, 1835. If *Globator* can be ruled type-genus of Globatoroida, then both the generic name and the Order-group name can be dealt with under the procedure laid down for *nomina dubia* in *Copenhagen Decisions*, paragraph 26.

Chronological list of names in the Order/Class-group in Echinoidea

Echinodermata Leske, 1778. Used as an unnamed category (above Order) for all echinoids, but not apparently including any other Echinoderms. The name is now used as the name of a Phylum and we do not propose to consider it in the Order/Class-group.

Echinus Leske, 1778. Listed as an Order. *Echinus* Linnaeus, 1758. Although Leske's name is homonymous with the name of the type-genus, it is clear that he adopted it as the name of a Linnean Order to include all echinoids in a sense quite distinct from his (and Linnaeus's) use of the generic name. We propose to adopt it, in the form Echinoidea, as the Class-name.

Catocysti Leske, 1778. Proposed as a " Class " below Order. *Echinocyamus* Van Phelsum, 1774. Subjective senior synonym of Clypeastridae A. Agassiz, 1873.

Pleurocysti Leske, 1778. Proposed as a " Class " below Order. *Spatangus* Leske, 1778. Objective senior synonym of Spatangidea Claus, 1876.

Echinodermata Wad, 1803. Proposed as a second-rank category under " Class Zoophyta ". See Echinodermata Leske, 1778.

Anocysti Parkinson, 1811. Proposed as a " Class " below Order. *Cidaris* Leske, 1778. Objective senior synonym of Cidarideae Claus, 1880.

Catocysti Parkinson, 1811. Proposed as a " grand division " below Order. *Echinocyamus* Van Phelsum, 1774. See Catocysti Leske, 1778.

Pleurocysti Parkinson, 1811. Proposed as a " Class " below Order. *Spatangus* Leske, 1778. See Pleurocysti Leske, 1778.

Echinodermia Rafinesque, 1815. Proposed as a Suborder. See Echinodermata Leske, 1778.

Radiaria Rafinesque, 1815. This name represents a taxonomic concept (that all animals possessing radial symmetry are directly related) which no longer claims any acceptance. It is suggested that specialists in the groups concerned should jointly petition the International Commission for its suppression.

Echini Goldfuss, 1820. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Echinida Fleming, 1822. Proposed as a " Tribe " under " Order I " which was not named. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Emmesostomi Parkinson, 1822. Proposed as a first-rank category (unnamed) below Order. *Cidaris* Leske, 1778. Objective senior synonym of Cidarideae Claus, 1880.

Apomesostomi Parkinson, 1822. Proposed as a first-rank category (unnamed) below Order. *Spatangus* Leske, 1778. Objective senior synonym of Spatangidea Claus, 1876.

Echinata Fischer von Waldheim, 1823. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Echinoderma Latreille, 1825. Proposed as a Class. See Echinodermata Leske, 1778.

Echinoida Latreille, 1825. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Echinides Stark, 1828. Proposed as a " Section " under Class Echinodermata. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Echinidae Fleming, 1828. Unnamed second-rank category below Order, but including all echinoids. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Anocysti Fleming, 1828. Proposed as a third-rank category (unnamed) below Order. *Cidaris* Leske, 1778. Objective senior synonym of Cidarideae Claus, 1880.

Catocysti Fleming, 1828. Proposed as a third-rank category (unnamed) below Order. *Echinocyamus* Van Phelsum, 1774. See Catocysti, Leske, 1778.

Pleurocysti Fleming, 1828. Proposed as a third-rank category (unnamed) below Order. *Spatangus* Leske, 1778. See Pleurocysti Leske, 1778.

Echinidea Blainville, 1834. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Pedicellata Griffith and Pidgeon, 1834. Proposed as an Order, to include both the Echinoidea and Asterozoa of current usage. This name should be transferred for consideration in the Phylum-group, as representing a Subphylum.

Cirrhi-Spinigrada Forbes, 1841. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Adelostella T. and T. Austin, 1842. Proposed as a Class. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Columnidae T. and T. Austin, 1842. Proposed as an Order. This originally included echinoids, crinoids, cystoids and blastoids and does not represent any useful taxonomic concept. In addition, it is a potential homonym of a Family-name in Gastropoda for the genus *Columna* Perry, 1811. Columnidae T. and T. Austin should be suppressed.

Echinidae T. and T. Austin, 1842. Described as an Order. *Echinus* Linnaeus, 1758. Objective senior synonym of Euechinoidea Bronn, 1860.

Perischoechinida M^cCoy, 1849. Described as an Order. *Palaechinus* M^cCoy, 1844. To be added to *Official List* as Subclass-name Perischoechinoidea.

Echinoidea d'Orbigny, 1852 and all subsequent usages. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778. This is the first use known to us of exactly this spelling (i.e. that currently used for the Class-name) for a taxon in the Order/Class-group including all Echinoids.

Perischoechinoidea Bronn, 1860. Proposed as an Order. *Palaechinus* M^cCoy. 1844. Objective junior synonym of Perischoechinida M^cCoy, 1849.

Euechinoidea Bronn, 1860. Proposed as an Order. *Echinus* Linnaeus, 1758. To be added to *Official List* as a Subclass-name.

Endocyclica Bronn, 1860. Proposed as a Suborder. *Echinus* Linnaeus, 1758. to be added to *Official List* as Subclass-name. This, with the following name, are the first latinized names known to us which represent the taxonomic concept usually expressed by the terms " Regularia " and " Irregularia ".

Exocyclica Bronn, 1860. Proposed as a Suborder. *Spatangus* Leske, 1778. To be added to *Official List* as Subclass-name.

Typica Carus, 1863. Proposed as a first-rank category (unnamed) below Class. *Echinus* Linnaeus, 1758. Objective junior synonym of Euechinoidea Bronn, 1860.

Regularia Carus, 1863. Proposed as a second-rank category (unnamed) below Class. *Echinus* Linnaeus, 1758. Objective junior synonym of Endocyclica Bronn, 1860.

Irregularia Carus, 1863. Proposed as a second-rank category (unnamed) below Class. *Spatangus* Leske, 1778. Objective junior synonym of Exocyclica Bronn, 1860.

Palechinida Haeckel, 1866. Proposed as a Subclass. *Palaechinus* M^{c}Coy, 1844. To be added to *Official List* as Order Palaechinoida.

Autechinida Haeckel, 1866. Proposed as a Subclass. *Echinus* Linnaeus, 1758. Objective junior synonym of Euechinoidea Bronn, 1860.

Melonitida Haeckel, 1866. Proposed as an Order. *Melonites* Norwood and Owen, 1846 (*non* Lamarck, 1822), replaced by *Melonechinus* Meek and Worthen, 1860. Since this name was formed from the name of an invalid generic homonym, it would be best considered stillborn. Since there are not as yet any rules that state that this must be the case, the best alternative is to suppress the name on the grounds that it has never been re-employed and is not used in any existing taxonomic scheme.

Eocidarida Haeckel, 1866. Proposed as an Order. *Palaechinus* M^{c}Coy, 1844. Objective junior synonym of Palechinida Haeckel (of which it was originally only a part).

Desmosticha Haeckel, 1866 and all subsequent usages. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Endocyclica Bronn, 1860.

Petalosticha Haeckel, 1866, and all subsequent usages. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Exocyclica Bronn, 1860.

Clypeastridae A. Agassiz, 1873. Proposed as a Suborder. *Clypeaster* Lamarck, 1801. To be added to *Official List* as Order Clypeasteroida. As explained elsewhere (Durham, 1955), the alteration of the stem of this name by the insertion of an e is considered a valid emendation on etymological grounds and in order to conform with all other names of the stem of which the Greek word ἀστὴe forms a part.

Clypeastridea Claus, 1876. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae A. Agassiz, 1873.

Spatangidea Claus, 1876. Proposed as an Order. *Spatangus* Leske, 1778. To be added to *Official List* as Order Spatangoida.

Palechinoidea Zittel, 1879 and all subsequent usages. Proposed as a Subclass. *Palaechinus* M^{c}Coy, 1844. Objective junior synonym of Perischoechinida M^{c}Coy, 1849.

Cystocidaridae Zittel, 1879. Proposed as an Order. *Cystocidaris* Zittel, 1879, an unnecessary nom. nov. for *Echinocystites* Wyville Thomson, 1861. The ordinal name is an objective senior synonym of Echinocystoida Jackson, 1912.

Bothriocidaridae Zittel, 1879. Proposed as an Order. *Bothriocidaris* Eichwald, 1869. To be added to *Official List* as Order Bothriocidaroida. This name is sometimes attributed to Schmidt, (F.), 1874, but he did not name the taxon, although he recognized the taxonomic need for one.

Perischoechinidae Zittel, 1879. Proposed as an Order. *Palaechinus* M^c^Coy, 1844. Objective junior synonym of Palechinida Haeckel, 1866.

Regulares Zittel, 1879. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Endocyclica Bronn, 1860. Zittel gives " Endocyclica Wright " as an alternative name ; this presumably refers to Echinoidea Endocyclica Wright, 1857, which is excluded from this list because it consists of more than one word.

Irregulares Zittel, 1879. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Exocyclica Bronn, 1860. Zittel's reference to " Exocyclica Wright " is ignored on the same grounds as are stated in the preceding entry.

Gnathostomata Zittel, 1879. Proposed as a Suborder. *Clypeaster* Lamarck, 1801. To be added to *Official List* as a Superorder.

Atelostomata Zittel, 1879. Proposed as a Suborder. *Spatangus* Leske, 1778. To be added to *Official List* as a Superorder.

Echinothurideae Claus, 1880. Proposed as a Suborder. *Echinothuria* Woodward, 1863. To be added to *Official List* as Order Echinothurioida.

Cidarideae Claus, 1880. Proposed as a Suborder. *Cidaris* Leske, 1778. To be added to *Official List* as Order Cidaroida.

Echinideae Claus, 1880. Proposed as a Suborder. *Echinus* Linnaeus, 1758. To be added to *Official List* as Order Echinoida (the nominate Order of Echinus Leske, 1778).

Clypeastroideae Claus, 1880. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae A. Agassiz, 1873.

Spatangoideae Claus, 1880. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Cassidulideae Claus, 1880. Proposed as a Suborder. *Cassidulus* Lamarck, 1801. To be added to *Official List* as Order Cassiduloida.

Spatangideae Claus, 1880. Proposed as a Suborder. *Spatangus* Leske, 1778. To be added to *Official List* as Suborder Spatangina.

Abranchiata Ludwig, 1882. Proposed as an Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Branchiata Ludwig, 1882. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinideae Claus, 1880.

Cidaridea Claus, 1883. Proposed as an Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Entobranchiata Ludwig, 1886. Proposed as a Suborder. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Ectobranchiata Ludwig, 1886. Proposed as a Suborder. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinideae Claus, 1880.

Clypeastroidea Ludwig, 1886. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae L. Agassiz, 1873.

Spatangoidea Ludwig, 1886. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Bothriocidaroida Duncan, 1889. Proposed as an Order. *Bothriocidaris* Eichwald, 1859. Objective junior synonym of Bothriocidaridae Zittel, 1879.

Cidaroida Duncan, 1889. Proposed as an Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Cystocidaroida Duncan, 1889. Proposed as an Order. See Cystocidaridae Zittel, 1879.

Plesiocidaroida Duncan, 1889. Proposed as an Order. *Tiarechinus* Neumayr, 1881. To be added to *Official List* as an Order.

Diadematoida Duncan, 1889. Proposed as an Order. *Diadema* Gray, 1825. To be added to *Official List* as Superorder Diadematacea and as name of its nominate Order.

Holectypoida Duncan, 1889. Proposed as an Order. *Holectypus* Desor, 1842. To be added to *Official List* as an Order.

Clypeastroida Duncan, 1889. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae A. Agassiz, 1873.

Cassiduloidea Duncan, 1889. Proposed as a Suborder. *Cassidulus* Lamarck, 1801. Objective junior synonym of Cassidulideae Claus, 1880.

Spatangoida Duncan, 1889. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Streptosomata Duncan, 1889. Proposed as a Suborder. *Echinothuria* S. Woodward, 1863. Objective junior synonym of Echinothurideae Claus, 1880.

Stereosomata Duncan, 1889. Proposed as a Suborder. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinideae Claus, 1880.

Palaeoechinida Perrier, 1893. Proposed as an Order. *Palaechinus* McCoy, 1844. Objective junior synonym of Perischoechinida McCoy, 1849.

Neoechinoidea Perrier, 1893. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Euechinoidea Bronn, 1860.

Cystechinida or Cystoechinoidea [sic] Haeckel, 1896. Proposed as a Subclass. See Cystocidaridae Zittel, 1879.

Promelonaria Haeckel, 1896. Proposed as an Order. *Cystocidaris* Zittel, 1879 (= *Echinocystites* Wyville Thomson, 1861). Objective senior synonym of Echinocystoida Jackson, 1912.

Eumelonaria Haeckel, 1896. Proposed as an Order. See Melonitida Haeckel, 1866.

Palechinida or Palaeoechinoidea or Perischoechinoidea [sic] Haeckel, 1896. Proposed as a Subclass. *Palaechinus* M^c^Coy, 1844. Objective junior synonyms of Perischoechinida M^c^Coy, 1849.

Stenopalmaria Haeckel, 1896. Proposed as an Order. *Bothriocidaris* Eichwald, 1859. Objective junior synonym of Bothriocidaridae Zittel, 1879.

Eurypalmaria Haeckel, 1896. Proposed as an Order. *Palaechinus* M^c^Coy, 1844. Objective junior synonym of Palechinida Haeckel, 1866.

Desmosticha = Cidaronia [sic] Haeckel, 1896. Proposed as a Legion or Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Cidaridaria Haeckel, 1896. Proposed as an Order or Suborder. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Diademaria Haeckel, 1896. Proposed as an Order or Suborder. *Diadema* Gray, 1825. Objective junior synonym of Diadematoida Duncan, 1889.

Anthosticha = Clypeastronia [sic] Haeckel, 1896. Proposed as a Legion or Order. *Clypeaster* Lamarck, 1801. Objective junior synonyms of Clypeastridae A. Agassiz, 1873.

Conoclyparia = Holectypida [sic] Haeckel, 1896. Proposed as an Order or Suborder. *Holectypus* Desor, 1842. Objective junior synonyms of Holectypoida Duncan, 1889.

Scutellaria Haeckel, 1896. Proposed as an Order or Suborder. *Scutella* Lamarck, 1816. To be added to *Official List* as Suborder Scutellina.

Petalosticha = Spatangonia [sic] Haeckel, 1896. Proposed as a Tribe or Order. *Spatangus* Leske, 1778. Objective junior synonyms of Spatangidea Claus, 1876.

Cassidularia or Cassiduloidea [sic] Haeckel, 1896. Proposed as an Order or Suborder. *Cassidulus* Lamarck, 1801. Objective junior synonyms of Cassidulideae Claus, 1880.

Spatangaria Haeckel, 1896. Proposed as an Order or Suborder. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Palaeo-echinoidea Parker and Haswell, 1897. Proposed as an Order. *Palaechinus* McCoy, 1844. Objective junior synonym of Perischoechinida McCoy, 1849.

Calycina Gregory, 1900. Proposed as a Suborder. *Salenia* Gray, 1835. To be added to *Official List* as Suborder.

Diademina Gregory, 1900. Proposed as a Suborder. *Diadema* Gray, 1825. To be added to *Official List* as Suborder.

Arbacina Gregory, 1900. Proposed as a Suborder. *Arbacia* Gray, 1835. To be added to *Official List* as Order-name Arbacioida and as name of its nominate Suborder.

Echinina Gregory, 1900. Proposed as a Suborder. *Echinus* Linnaeus, 1758. To be added to *Official List* as Suborder.

Holectypina Gregory, 1900. Proposed as a Suborder. *Holectypus* Desor, 1842. To be added to *Official List* as Suborder.

Clypeastrina Gregory, 1900. Proposed as a Suborder. *Clypeaster* Lamarck, 1801. To be added to *Official List* as Suborder-name Clypeasterina.

Asternata Gregory, 1900. Proposed as a Suborder. *Cassidulus* Lamarck, 1801. Objective junior synonym of Cassidulideae Claus, 1880.

Sternata Gregory, 1900. Proposed as a Suborder. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Gnathostomata Lambert, 1900 (*non* Zittel, 1879). Proposed as a Subclass. *Echinus* Linnaeus, 1758. Objective junior synonym of Euechinoidea Bronn, 1860. Homonym of Gnathostomata Zittel, 1879.

Plagiocysta Lambert, 1900. Proposed as an Order. *Echinocystites* Wyville Thomson, 1861. Objective senior synonym of Echinocystoida Jackson, 1912.

Pileatoida Lambert, 1900. Proposed as a Suborder. *Holectypus* Desor, 1842. Objective junior synonym of Holectypoida Duncan, 1889.

Exocysta Lambert, 1900. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae A. Agassiz, 1873.

Stereodermata Lambert, 1900. Proposed as an Order. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinideae Claus, 1880.

Echinidia Delage and Hérouard, 1903. Proposed as a Class. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinus Leske, 1778.

Regulariae Delage and Hérouard, 1903. Proposed as a Subclass. *Echinus* Linnaeus, 1758. Objective junior synonym of Endocyclica Bronn, 1860.

Irregulariae Delage and Hérouard, 1903. Proposed as a Subclass. *Spatangus* Leske, 1778. Objective junior synonym of Exocyclica Bronn, 1860.

Echinothurida Delage and Hérouard, 1903. Proposed as an Order. *Echinothuria* S. Woodward, 1863. Objective junior synonym of Echinothurideae Claus, 1880.

Cidarida Delage and Hérouard, 1903. Proposed as an Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Diademida Delage and Hérouard, 1903. Proposed as an Order. *Diadema* Gray, 1825. Objective junior synonym of Diadematoida Duncan, 1889.

Diademina Delage and Hérouard, 1903. Proposed as a Tribe (below Order). *Diadema* Gray, 1825. Objective junior synonym of Diademina Gregory, 1900.

Salenina Delage and Hérouard, 1903 (as Salelina in Contents). Proposed as a Tribe (below Order). *Salenia* Gray, 1835. Objective junior synonym of Calycina Gregory, 1900.

Cyphosomina Delage and Hérouard, 1903. Proposed as a Tribe (below Order). *Cyphosoma* L. Agassiz, 1838 (*non* Mannerheim, 1837) = *Phymosoma* Haime, 1853. Invalid senior objective synonym of Phymosomina Mortensen, 1904.

Holectypida Delage and Hérouard, 1903. Proposed as an Order. *Holectypus* Desor, 1842. Objective junior synonym of Holectypoida Duncan, 1889.

Clypeastrida Delage and Hérouard, 1903. Proposed as an Order. *Clypeaster* Lamarck, 1801. Objective junior synonym of Clypeastridae A. Agassiz, 1873.

Spatangida Delage and Hérouard, 1903. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Spatangidea Claus, 1876.

Endobranchiata Meissner, 1903. Unnamed category "A I" equivalent to Order. *Cidaris* Leske, 1778. Objective junior synonym of Cidarideae Claus, 1880.

Phymosomina Mortensen, 1904. Proposed as a Suborder. *Phymosoma* Haime, 1853. To be added to *Official List* as Order Phymosomatoida.

Protosternata Mortensen, 1907. Proposed as a Suborder. *Collyrites* Desmoulins, 1835. To be added to *Official List*.

Meridosternata Mortensen, 1907. Proposed as a Suborder. *Holaster* L. Agassiz, 1836. To be added to *Official List*.

Amphisternata Mortensen, 1907. Proposed as a Suborder. *Spatangus* Leske, 1778. To be added to *Official List*.

Echinocystoida Jackson, 1912. Proposed as an Order. *Echinocystites* Wyville Thomson, 1861. To be added to *Official List* as Echinocystitoida (to conform with stem of name of type-genus).

Centrechinoida Jackson, 1912. Proposed as an Order. *Diadema* Gray, 1825. Objective junior synonym of Diadematoida Duncan, 1889.

Aulodonta Jackson, 1912. Proposed as a Suborder. *Diadema* Gray, 1825. Objective junior synonym of Diademina Gregory, 1900.

Stirodonta Jackson, 1912. Proposed as a Suborder. *Phymosoma* Haime, 1853. Objective junior synonym of Phymosomina Mortensen, 1904.

Camarodonta Jackson, 1912. Proposed as a Suborder. *Echinus* Linnaeus, 1758. Objective junior synonym of Echinina Gregory, 1900.

Exocycloida Jackson, 1912. Proposed as an Order. *Spatangus* Leske, 1778. Objective junior synonym of Exocyclica Bronn, 1860.

Brachygnata Lambert, 1915. Proposed as an Order. *Echinoneus* Leske, 1778, by monotypy. Objective senior synonym of Echinoneina Clark, 1925.

Globatoroida Lambert, 1915. Proposed as a Suborder. *Echinoneus* Leske, 1778, by monotypy. Objective senior synonym of Echinoneina Clark, 1925.

Nodostomata Lambert, 1915. Proposed as an Order. *Amblypygus* L. Agassiz, 1840, by monotypy. Subjective junior synonym of Cassidulideae Claus, 1880.

Procassiduloida Lambert, 1918, ex Lambert, 1915, (*nomen nudum*). Proposed as a Suborder. *Procassidulus* Lambert and Thiéry, 1918. Subjective junior synonym of Cassidulideae Claus, 1880.

Nucleolitoida Hawkins, 1920. Proposed as an Order. *Nucleolites* Lamarck, 1801. To be added to *Official List*.

Echinoneina H. L. Clark, 1925. Proposed as a Suborder. *Echinoneus* Leske, 1778. To be added to *Official List*.

Melonechinoida Mortensen, 1934. Proposed as an Order. *Palaechinus* M^{c}Coy, 1844. Objective junior synonym of Palechinida Haeckel, 1866.

Lepidocentroida Mortensen, 1934. Proposed as an Order. *Echinocystites* Wyville Thomson, 1861. Objective junior synonym of Echinocystoida Jackson, 1912.

Pseudoechinoidea Mortensen, 1935. Proposed as a Subclass. *Bothriocidaris* Eichwald, 1859. To be added to *Official List*.

Hemicidarina Beurlen, 1937. Proposed as a Suborder. *Hemicidaris* L. Agassiz, 1838. To be added to *Official List* as Order Hemicidaroida.

Megalopoda Macbride and Spencer, 1938. *Eothuria* Macbride and Spencer, 1938, which is not certainly an echinoid.

Aspidodiademina Mortensen, 1939. Proposed as a Suborder. *Aspidodiadema* A. Agassiz, 1879. To be added to *Official List*.

Pedinina Mortensen, 1939. Proposed as a Suborder. *Pedina* L. Agassiz, 1838. To be added to *Official List.*

Orthopsina Mortensen, 1942. Proposed as a Suborder. *Orthopsis* Cotteau, 1864. To be added to *Official List.*

Temnopleurina Mortensen, 1942. Proposed as a Suborder. *Temnopleurus* Duncan, 1889. To be added to *Official List* as Order Temnopleuroida.

Cassidulina Mortensen, 1948. Proposed as a Suborder. *Cassidulus* Lamarck, 1801. Objective junior synonym of Cassidulideae Claus, 1880.

Conoclypina Mortensen, 1948. Proposed as a Suborder. *Conoclypus* L. Agassiz, 1839. To be added to *Official List.*

Laganina Mortensen, 1948. Proposed as a Suborder. *Laganum* Link, 1807. To be added to *Official List.*

Scutellina Durham, 1955. Proposed as a Suborder. *Scutella* Lamarck, 1816. Objective junior synonym of Scutellaria Haeckel, 1896.

Rotulina Durham, 1955. Proposed as a Suborder. *Rotula* Schumacher, 1817. To be added to *Official List.*

Holasteroida Durham and Melville, 1957. Proposed as an Order. *Holaster* L. Agassiz, 1836. To be added to *Official List.*

Alphabetical List of Names in the Order/Class-Group in the Class Echinoidea

Abranchiata Ludwig, 1882
Adelostella T. and T. Austin, 1842
Amphisternata Mortensen, 1907
Anocysti Parkinson, 1811
Anocysti Fleming, 1828
Anthosticha Haeckel, 1896
Apomesostomi Parkinson, 1822
Arbacina Gregory, 1900
Aspidodiademina Mortensen, 1939
Asternata Gregory, 1900
Atelostomata Zittel, 1879
Aulodonta Jackson, 1912
Autechinida Haeckel, 1866
Bothriocidaridae Zittel, 1879
Bothriocidaroida Duncan, 1889
Branchiata Ludwig, 1882
Brachygnata Lambert, 1915
Calycina Gregory, 1900
Camarodonta Jackson, 1912
Cassidularia Haeckel, 1896
Cassidulideae Claus, 1880
Cassidulina Mortensen, 1948
Cassiduloidea Duncan, 1889
Catocysti Leske, 1778
Catocysti Parkinson, 1811
Catocysti Fleming, 1828
Centrechinoida Jackson, 1912
Cidarida Delage and Hérouard, 1903
Cidaridaria Haeckel, 1896
Cidaridea Claus, 1883

Cidarideae Claus, 1880
Cidaroida Duncan, 1889
Cidaronia Haeckel, 1896
Cirrhi-Spinigrada Forbes, 1841
Clypeastrida Delage and Hérouard, 1903
Clypeastridae A. Agassiz, 1873
Clypeastridea Claus, 1876
Clypeastrina Gregory, 1900
Clypeastroida Duncan, 1889
Clypeastroidea Ludwig, 1886
Clypeastroideae Claus, 1880
Clypeastronia Haeckel, 1896
Columnidae T. and T. Austin, 1842
Conoclyparia Haeckel, 1896
Conoclypina Mortensen, 1948
Cyphosomina Delage and Hérouard, 1903
Cystechinida Haeckel, 1896
Cystocidaridae Zittel, 1879
Cystocidaroida Duncan, 1889
Cystoechinoidea Haeckel, 1896
Desmosticha Haeckel, 1866
Diademaria Haeckel, 1896
Diadematoida Duncan, 1889
Diademida Delage and Hérouard, 1903
Diademina Gregory, 1900
Echinata Fischer de Waldheim, 1823
Echini Goldfuss, 1820
Echinida Fleming, 1822
Echinidae Fleming, 1828
Echinidae T. and T. Austin, 1842
Echinidea Blainville, 1834
Echinidea Haeckel, 1896
Echinideae Claus, 1880
Echinides Stark, 1828
Echinidia Delage and Hérouard, 1903
Echinina Gregory, 1900
Echinocystoida Jackson, 1912
Echinoderma Latreille, 1825
Echinodermata Leske, 1778
Echinodermata Wad, 1803
Echinodermia Rafinesque, 1815
Echinoida Latreille, 1825
Echinoidea d'Orbigny, 1842
Echinoneina H. L. Clark, 1925
Echinothurida Delage and Hérouard, 1903
Echinothurideae Claus, 1880
Echinus Leske, 1778
Ectobranchiata Ludwig, 1886
Emmesostomi Parkinson, 1822
Endobranchiata Meissner, 1903
Endocyclica Bronn, 1860
Entobranchiata Ludwig, 1886
Eocidarida Haeckel, 1866
Euechinoidea Bronn, 1860
Eumelonaria Haeckel, 1896
Eurypalmaria Haeckel, 1896
Exocyclica Bronn, 1860
Exocycloida Jackson, 1912
Exocysta Lambert, 1900
Globatoroida Lambert, 1915
Gnathostomata Zittel, 1879
Gnathostomata Lambert, 1900 (*non* Zittel, 1879)
Hemicidarina Beurlen, 1937
Holasteroida Durham and Melville, 1957
Holectypida Haeckel, 1896
Holectypida Delage and Hérouard, 1903

Holectypina Gregory, 1900
Holectypoida Duncan, 1889
Irregulares Zittel, 1879
Irregularia Carus, 1863
Irregulariae Delage and Hérouard, 1903
Laganina Mortensen, 1948
Lepidocentroida Mortensen, 1934
Megalopoda MacBride and Spencer, 1938
Melonechinoida Mortensen, 1934
Melonitida Haeckel, 1866
Meridosternata Mortensen, 1907
Neoechinoidea Perrier, 1893
Nodostomata Lambert, 1915
Nucleolitoida Hawkins, 1920
Orthopsina Mortensen, 1942
Palaeoechinida Perrier, 1893
Palaeo-echinoidea Parker and Haswell, 1897
Palechinida Haeckel, 1866
Palechinida or Palaeoechinoidea or Perischoechinoidea Haeckel, 1896
Palechinoidea Zittel, 1879
Pedicellata Griffith and Pidgeon, 1834
Pedinina Mortensen, 1939
Perischoechinida M^cCoy, 1849
Perischoechinidae Zittel, 1879
Perischoechinoidea Bronn, 1860
Petalosticha Haeckel, 1866
Phymosomina Mortensen, 1904
Pileatoida Lambert, 1900
Plagiocysta Lambert, 1900
Plesiocidaroida Duncan, 1889
Pleurocysti Leske, 1778
Pleurocysti Parkinson, 1811
Pleurocysti Fleming, 1828
Procassiduloida Lambert, 1918
Promelonaria Haeckel, 1896
Protosternata Mortensen, 1907
Pseudoechinoidea Mortensen, 1935
Radiaria Rafinesque, 1815
Regulares Zittel, 1879
Regularia Carus, 1863
Regulariae Delage and Hérouard, 1903
Rotulina Durham, 1955
Salelina Delage and Hérouard, 1903
Salenina Delage and Hérouard, 1903
Scutellaria Haeckel, 1896
Scutellina Durham, 1955
Spatangaria Haeckel, 1896
Spatangida Delage and Hérouard, 1903
Spatangidea Claus, 1876
Spatangideae Claus, 1880
Spatangoida Duncan, 1889
Spatangoidea Ludwig, 1886
Spatangoidea Duncan, 1889
Spatangoideae Claus, 1880
Spatangonia Haeckel, 1896
Stenopalmaria Haeckel, 1896
Stereodermata Lambert, 1900
Stereosomata Duncan, 1889
Sternata Gregory, 1900
Stirodonta Jackson, 1912
Streptosomata Duncan, 1889
Temnopleurina Mortensen, 1942
Typica Carus, 1863

Available Names in the Order/Class-Group in Echinoidea

Class

Echinoidea Leske, 1778 (as Echinus). Objective junior synonyms Echini Goldfuss, 1820, Echinida Fleming, 1822, Echinata Fischer von Waldheim, 1823, Echinoida Latreille, 1825, Echinidae Fleming, 1828, Echinides Stark, 1828, Echinidea Blainville, 1834, Cirrhi-Spinigrada Forbes,1841, Adelostella T. and T. Austin, 1842, Echinoidea d'Orbigny, 1852, Echinidia Delage and Hérouard, 1903.

Subclass

Endocyclica Bronn, 1860. Objective junior synonyms Regularia Carus, 1863, Desmosticha Haeckel, 1866, Regulares Zittel, 1879, Regulariae Delage and Hérouard, 1903.

Euechinoidea Bronn, 1860. Objective senior synonym, Echinidae T. and T. Austin, 1842. Objective junior synonyms Typica Carus, 1863, Autechinida Haeckel, 1866, Neoechinoidea Perrier, 1893, Gnathostomata Lambert, 1900 (*non* Zittel, 1879).

Exocyclica Bronn, 1860. Objective junior synonyms Irregularia Carus, 1863, Petalosticha Haeckel, 1866, Irregulares Zittel, 1879, Irregulariae Delage and Hérouard, 1903, Exocycloida Jackson, 1912.

Perischoechinoidea M^cCoy, 1849 (as Perischoechinida). Objective junior synonyms Perischoechinoidea Bronn, 1860, Palechinoidea Zittel, 1879, Palaeoechinida Perrier, 1893, "Palechinida or Palaeoechinoidea or Perischoechinoidea" Haeckel, 1896.

Pseudoechinoidea Mortensen, 1935.

Superorder

Atelostomata Zittel, 1879

Diadematacea Duncan, 1889 (as Diadematoida)

Echinacea Claus, 1880 (as Echinideae)

Gnathostomata Zittel, 1879.

Order

Arbacioida Gregory, 1900.

Bothriocidaroida Zittel, 1879 (as Bothriocidaridae). Objective junior synonyms Bothriocidaroida Duncan, 1889, Stenopalmaria Haeckel, 1896.

Cassiduloida Claus, 1880 (as Cassidulideae). Objective junior synonyms Cassiduloidea Duncan, 1889, Cassidularia Haeckel, 1896, Asternata Gregory, 1900, Procassiduloida Lambert and Thiéry, 1921 ; subjective junior synonym Nodostomata Lambert, 1915.

Cidaroida Claus, 1880 (as Cidarideae). Objective senior synonyms Anocysti Parkinson, 1822 and Fleming, 1828, Emmesostomi Parkinson, 1822 ; objective junior synonyms Abranchiata Ludwig, 1882, Cidaridea Claus, 1883, Entobranchiata Ludwig, 1886, Cidaroida Duncan, 1889, Desmosticha Haeckel, 1896, Cidaronia Haeckel, 1896, Cidaridaria Haeckel, 1896, Cidarida Delage and Hérouard, 1903, Endobranchiata Meissner, 1903.

Clypeasteroida A. Agassiz, 1873 (as Clypeastridae). Subjective senior synonyms Catocysti Leske, 1778, Parkinson, 1811 and Fleming, 1828 ; objective junior synonyms Clypeastridea Claus, 1876, Clypeastroideae Claus, 1880, Clypeastroidea Ludwig, 1882, Clypeastroida Duncan, 1889, Anthosticha Haeckel, 1896, Clypeastronia Haeckel, 1896, Exocysta Lambert, 1900, Clypeastrida Delage and Hérouard, 1903.

Diadematoida Duncan, 1889. Objective junior synonyms Diademaria Haeckel, 1896, Diademida Delage and Hérouard, 1903, Centrechinoida Jackson, 1912.

Echinocystitoida Jackson, 1912 (as Echinocystoida). Objective senior synonyms Cystocidaridae Zittel, 1879, Cystocidaroida Duncan, 1889, Promelonaria Haeckel, 1896, Cystechinida-Cystoechinoidea Haeckel, 1896, Plagiocysta Lambert, 1900 ; Objective junior synonym Lepidocentroida Mortensen, 1934.

Echinoida Claus, 1880 (as Echinideae). Objective junior synonyms Branchiata Ludwig, 1882, Ectobranchiata Ludwig, 1886, Stereosomata Duncan, 1889, Stereodermata Lambert, 1900.

Echinothurioida Claus, 1880 (as Echinothurideae). Objective junior synonyms Streptosomata Duncan, 1889, Echinothurida Delage and Hérouard, 1903.

Hemicidaroida Beurlen, 1937.

Holasteroida Durham and Melville, 1957.

Holectypoida Duncan, 1889. Objective junior synonyms Conoclyparia Haeckel, 1896, Holectypida Delage and Hérouard, 1903, Pileatoida Lambert, 1900 ; Subjective junior synonyms Brachygnata Lambert, 1915, Globatoroida Lambert and Thiéry, 1921.

Megalopoda MacBride and Spencer, 1938.

Nucleolitoida Hawkins, 1920.

Palaechinoida Haeckel, 1866 (as Palechinida). Objective junior synonyms Eocidarida Haeckel, 1866, Perischoechinidae Zittel, 1879, Eurypalmaria Haeckel, 1896, Palaeo-echinoidea Parker and Haswell, 1897, Melonechinoida Mortensen, 1934.

Phymosomatoida Mortensen, 1904 (as Phymosomina). Objective senior synonym Cyphosomina Delage and Hérouard, 1903 ; objective junior synonym Stirodonta Jackson, 1912.

Plesiocidaroida Duncan, 1889.

Spatangoida Claus, 1876 (as Spatangidea). Objective senior synonyms Pleurocysti Leske, 1778, Parkinson, 1811, Fleming, 1828, Apomesostomi Parkinson, 1822 ; objective junior synonyms Spatangideae Claus, 1880, Spatangoideae Claus, 1880, Spatangoida Duncan, 1889, Petalosticha Haeckel, 1896, Spatangonia Haeckel, 1896, Spatangaria Haeckel, 1896, Sternata Gregory, 1900, Spatangida Delage and Hérouard, 1903.

Temnopleuroida Mortensen, 1942.

Suborder

Amphisternata Mortensen, 1937.

Aspidodiademina Mortensen, 1939.

Calycina Gregory, 1900. Objective junior synonym Salenina ("Salelina") Delage and Hérouard, 1903.

Cassidulina *sensu stricto* (Mortensen, 1948).

Clypeasterina *sensu stricto* (Gregory, 1900, as Clypeastrina).

Conoclypina Mortensen, 1948.

Diademina *sensu stricto* (Gregory, 1900). Objective junior synonym Aulodonta Jackson, 1912.

Echinina *sensu stricto* (Gregory, 1900). Objective junior synonym Camarodonta Jackson, 1912.

Echinoneina H. L. Clark, 1925. Objective senior synonyms Brachygnata Lambert, 1915, Globatoroida Lambert, 1915.

Holectypina *sensu stricto* (Gregory, 1900).

Laganina Mortensen, 1948.

Meridosternata Mortensen, 1907.

Orthopsina Mortensen, 1942.

Pedinina Mortensen, 1939.

Protosternata Mortensen, 1907.

Rotulina Durham, 1955.

Scutellina Haeckel, 1896 (as Scutellaria).

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DOCUMENT 25/3

Proposed type genera for higher taxa within the Sub-Phylum Ciliophora (Phylum Protozoa)

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(Enclosure to a letter dated 14th December 1957)[1]

Acting within the spirit of the *Copenhagen Decisions on Zoological Nomenclature* (Hemming, 1953) and appreciating the need to stabilize the higher zoological taxa by fixation of type genera, the present paper considers the problem of selecting such types in a subphylum of " lower organisms ", the ciliated protozoa.

2. Before types can be designated, or even suggested, another nomenclatural problem should be resolved as satisfactorily as possible : clarification of the standing of names of the taxa themselves in the Order/Class-Group within the subphylum Ciliophora. Classificational schemes are also involved to the extent that various groups have been shifted in their rank and relationship to each other by different authors. Strictly nomenclatural aspects of ciliate systematics, however, are for the most part unaffected by the taxonomic position of the various groups, if proposed changes in rank have not been too drastic.

3. Various classificational schemes are currently in use for the ciliates. One very recent revision has just been published (Corliss, 1956, 1957), based principally upon proposals made by Fauré-Fremiet (1950). Several major differences exist between conventional schemes and the latest rearrangement. The suggested alterations are based upon analyses and interpretations of new information or of data considered more reliable (i.e., more fundamental in nature) from a phylogenetic point of view than those generally employed.

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[1] This case has an associated Registered Number Z.N.(S.) 1302.

4. The propositions of the *Copenhagen Decisions* related to nomenclatural procedures for higher taxa have been applied to group names within the Ciliophora (Corliss, 1957). This has revealed that most of the existing confusion stems from past failures to recognize priority at the higher taxonomic levels. Plenary Powers of the International Commission on Zoological Nomenclature must be invoked to resolve cases of homonomy and to preserve several time-honored names.

5. A more detailed proposal of type genera for the higher ciliate groups will be made to the International Commission on Zoological Nomenclature in proper form at a later date ; but the following list of suggested names is essentially a condensation of the information to be included in such a petition. Thus the present paper is not an attempt to offer formal designation of types. Shifting various systematic groups to higher or lower ranks in the classificational scheme in general need not affect the relationship of a taxon to its type genus ; thus a worthwhile degree of stability can be achieved by suggesting type genera, even though the scheme adopted here as a framework may undergo subsequent change. The thirty taxonomic units recognised below involve twenty-one different genera of ciliates as types. The genera chosen are generally quite central taxonomically to the collection of subunits involved. In addition the selected genera, with two exceptions, are also types of representative families within the orders or suborders concerned.

6. For conciseness the following plan of presentation is used below : the name of a given taxon includes only the (original) author, not the date ; this information is separated from the full name of the type genus by a colon. Names of authors are abbreviated following their first usage.

7. Subphylum Ciliophora Doflein : *Paramecium* O. F. Müller, 1773 ; same type genus for the single class Ciliata Perty, and for the subclass Holotricha Stein. Order Gymnostomatida Bütschli : *Holophrya* Ehrenberg, 1831 ; suborder Rhabdophorina Fauré-Fremiet : same as for the order ; suborder Cyrtophorina F-F : *Nassula* Ehrbg., 1833. Order Suctorida Claparède & Lachmann : *Acineta* Ehrbg., 1833. Order Chonotrichida Wallengren : *Spirochona* Stein, 1852. Order Trichostomatida Büt. : *Colpoda* O.F.M., 1773. Order Hymenostomatida Delage & Hérouard : *Paramecium* O.F.M., 1773 ; suborder Tetrahymenina F-F : *Tetrahymena* Furgason, 1940 ; suborder Peniculina F-F : same as for the order ; suborder Pleuronematina F-F : *Pleuronema* Dujardin, 1841. Order Astomatida Schewiakoff : *Anoplophrya* Stein, 1860. Order Apostomatida Chatton & Lwoff : *Foettingeria* Caullery & Mesnil, 1903. Order Thigmotrichida Ch. & Lw. : *Hemispeira* Fabre-Domergue, 1888 ; suborder Arhynchodina Corliss : same as for the order ; suborder Rhynchodina Ch. & Lw. : *Ancistrocoma* Ch. & Lw., 1926. Order Peritrichida Stein : *Vorticella* Linnaeus, 1766 ; suborder Sessilina Kahl : *Urceolaria* Lamarck, 1801 ; suborder Mobilina Kahl : same as for the order.

8. Subclass Spirotricha Büt. : *Euplotes* Ehrbg., 1831. Order Heterotrichida Stein : *Condylostoma* Bory, 1826 ; suborder Heterotrichina Cor. : same as for the order ; suborder Licnophorina Cor. : *Licnophora* Claparède, 1867. Order Oligotrichida Büt. : *Halteria* Duj., 1840. Order Tintinnida Kofoid & Campbell : *Tintinnus* Schrank, 1803. Order Entodiniomorphida Reichenow : *Entodinium* Stein, 1858. Order Odontostomatida Sawaya [replacing Ctenostomatida Kahl, preoccupied] : *Saprodinium* Lauterborn, 1908. Order Hypotrichida Stein : *Euplotes* Ehrbg., 1831.

9. Rather detailed discussion of certain choices of generic names used above is warranted but is beyond the scope of the present abbreviated report. It may be interesting to note in passing that no genus of ciliates was listed in Linnaeus' 10th edition of the *Systema Naturae*, although Hill (1752) had published the name " *Paramecium* " six years earlier. Rarely have types been designated for any of the ciliate taxa ; the relationship of such cases to the proposals made above also will be considered in a fuller paper to be published elsewhere at an appropriate later date.

DOCUMENT 25/4

Questions arising in connection with the naming of Orders and taxa of Higher Rank

By **ELLSWORTH C. DOUGHERTY**

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(**Editorial Note :** In a letter dated 20th November 1957 Dr. Dougherty explained that, in conjunction with Dr. Benjamin G. Chitwood, he had recently been engaged on work on a re-classification of the Nematodes and that in the course of this work Dr. Chitwood and he had found themselves in disagreement on certain questions relating to the naming of Orders and taxa of higher rank. Dr. Dougherty explained that he had set out his views in a series of documents which had formed enclosures to a letter which he had recently addressed to Dr. Chitwood. These papers, he suggested, might be published in the *Bulletin of Zoological Nomenclature*. It has been judged that the most convenient course would be to present this documentation to the London Congress for consideration in connection with Section 1 of Article 12 of the Draft *Règles* (1957, *Bull. zool. Nomencl.* **14** : 92). The following extract from Dr. Dougherty's letter, together with the enclosures to that letter, has accordingly been allotted the Congress Number Document 25/4 and is reproduced below. (Intl'd. F.H. 23rd January 1957)

(Extract from a letter, with enclosure, dated 20th November 1957)

. . . .

Dr. Chitwood and I have reached a fundamental impasse with respect to the criteria by which names of higher taxa of the Order/Class and Phylum Groups are to be reckoned for the purposes of the Law of Priority. I am enclosing some appendices (II–V) to a recent letter to Dr. Chitwood. If any of this material seems suitable for the *Bulletin* (with appropriate recasting, of course), please let me know.

In the first appendix of my letter to Dr. Chitwood (of which an extra copy was not made), I indicated that I planned to send you copies of Appendices II and III ; subsequently to writing that, however, I recast the material a bit so that it came to be four appendices, instead of but two. A copy of this letter goes to Dr. Chitwood by way of explanation of this fact.

APPENDIX I

(not furnished to the Office of the Commission by Dr. Dougherty)

APPENDIX II

Determination of Names for Higher Zoological Taxa

A. Present Rules

In our recent letters we have been wrestling with problems that, in important respects, Copenhagen left unsolved. The volume *Copenhagen Decisions* (1953) has a Section D (pp. 38–43), which is entitled : " Proposed adoption of rules for the naming of Orders and Higher Taxonomic Categories ".

One thing is immediately evident : the scheme for arriving at lists of recommended names for taxa of the Order/Class- and Phylum-Groups in the Animal Kingdom, as outlined in Decision 62 (pp. 38–40), has yet to be implemented. Certainly the suggestion (Decision 62(6)) that " Specialist Committees " have such lists ready and published before " the Linnean Bicentenary in 1958 " was hopelessly optimistic. No one at the Copenhagen Colloquium gave any indication of understanding the enormity of the problems involved. (Certainly I did not realize it ; only through our joint efforts, in fact, have I come to appreciate fully this situation.) The fact that, to my knowledge, Francis Hemming has not actively sought to have " Committees of Specialists " formed is, I feel, partly due to the intrinsic difficulties involved.[1] No doubt another factor has also played a critical role : he has, I believe, been overwhelmed with a flood of problems of all sorts, whose extent the Colloquium also failed to recognize. The implementation of the Copenhagen provisions with respect to names of higher taxa (i.e., those above the Family-Group) has, I surmise, been forced to a position of relatively low priority by the imperative nature of more urgent problems. The participants of the forthcoming London Colloquium will, I am sure, have a more realistic understanding of the time necessary for the realization of the goals set at Copenhagen (some of which will, I believe, be modified).

But, if I start with the body of law enacted in 1953 for deciding on names of higher taxa, I can, I feel, illuminate some of our problems rather more adequately than has been done so far by either of us.

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[1] See Document 25/1, paragraph 3.

As regards higher-taxon names already proposed, I draw your attention first of all to main Decisions 63 (p. 38) and 62(1) (pp. 31–34) and quote from the following : " [Decision 62] The Colloquium recommends . . . Decision 62(2) . . . [that the] Commission should be asked to invite the Committees of Specialists, when selecting names to be included in the recommended lists, to give first consideration to weight of current usage, and, when usage affords no clear basis for choice, to other considerations, such as priority . . . ". This is the ruling on which I base my preference for Nematoda as a Class or Phylum name. It implies, of course, a " popularity contest ", of which you have been bitterly critical. If enough other zoologists feel as you do, this rule can be changed. I happen to agree with it on principle ; but I fully realize that its application has many pitfalls.

Let us, for the sake of argument, decide that this is not a good rule—that, instead, some rule based on the Law of Priority should be substituted for it. Now we are faced with the problem of just what way in which to formulate such a rule.

Over the past months you have gradually clarified your thinking on these issues—partly, I am sure, under pressure from me. I believe that you have been primarily drawn to your present position by your desire to preserve Nemata as the phylar name for the nemas (=nematodes) and that most other aspects of the problem have been subordinated to this aim.

B. Your Proposal for a Rule on Acceptance of Names of Higher Taxa

In your last letter (of Nov. 14th) you enunciated a principle (hereinafter referred to as " [your] Rule ") that is quite clear—I quote : " The only formula we can arrive at is that the stem of the name used by the man who made the final logical exclusions should be accepted if he used that name at the rank we use today ". At first glance, this would seem a fair enough rule, but, if ever formalized, it could lead to endless confusion as I can immediately show.

Before going into the more important objections, I should point out that, from your standpoint, it would have one effect that you may not have perceived and would, I believe, not wish : namely, it would ensure the preservation of a name with the stem *aphasmid-*, whether at the Subclass or Class level, for certainly Aphasmidia (subclass) and Aphasmidea (class) are the first names used for taxonomically rational taxa at those levels (Adenophori having followed Aphasmidia at the Subclass level and Anenophorea being far junior to the Aphasmidea at the Class level). (Secernentea as a Class name would, on the other hand be valid under your Rule since it would be a replacement for the homonymous name Phasmidea.)

The difficulties with your Rule are more deep-seated than this, however ; they derive from two main facts. First, its implementation would require an intrusion of taxonomy into nomenclature far more than any formal provision of the Rules now provides for—with the all-important exception that, fundamentally, the " popularity contest " provision, of which you are so critical, would allow full operation of taxonomic (or systematic, if you will) ideas to operate in reaching ultimate decisions on nomenclature. (Incidentally, this principle is also extended to names of the Family-Group—see Decision 45 [p. 33].) Second, your Rule would require junking what I should call the " Principle of Co-ordination " of taxa of the Order/Class- and Phylum-Groups (see Decision 66 [pp. 41–42]).

C. Nomenclature *vs.* Taxonomy

One of the most difficult problems in the nomenclatural aspect of systematics at the level of higher taxa is that of deciding to what extent use of names should vary according to taxonomic concepts. Given the *Règles* as amended at Copenhagen and general zoological traditions in nomenclature, going far back of Copenhagen, I contend that one should strive, in settling on rules of nomenclature for higher taxa, to minimize the obtrusion of taxonomic concepts.

Your Rule could, however, embroil zoology in an endless confusion in many cases. In effect, it would abolish the fundamental usefulness of the type system for higher taxa (type genera in these cases). You may be inclined, I should anticipate, to dispute that it in any way would interfere with the type system, but I think that it can be fairly shown that it would. Types are useful primarily as anchors for names when there are, as is inevitable in our growing state of taxonomic knowledge, changes in taxonomic systems.

Let us examine what would be the full implication of a rule requiring that that name be used that was first applied to a group after " logical exclusions " [= a " natural " group] had been made. This brings us full-tilt into taxonomic issues that I feel strongly should be left out of nomenclature.

Look what would happen in an extreme case if it were necessary (as it would be in the strict application of your Rule) to accept any name change associated with a change of content in a given taxon ; in such cases, it is obvious that the type would stand for little. Let us imagine a higher taxon *X* of a given rank that, when originally named, contained subordinate taxa (orders, families, genera—it doesn't matter much for the sake of this discussion which they were) ; let us call these subordinate taxa A, B, C, D, E. Now, by your Rule, any of the following would result in a condition in which a different name, if proposed, would be binding :

(1) A, B, C, D, with E removed (with name change of group to *Y*) ;

(2) A, B, C, with D and E removed (with name change to group to *Z*) ;

(3) and so on through many permutations.

A worker accepting concept (1) would be bound by name *Y* ; a worker accepting concept (2) would be bound by name *Z* ; etc. Similarly, the introduction of any other subordinate taxon (F, G, etc.) into *X* would require that any name-change undergone by *X* be binding. The ultimate consequences of this are ridiculous. And what is a valid group anyway ? In fact, who are we to say that a group is valid ?

No, I think we need the type system for higher taxa, just as we do for species, genera, and families and taxa subordinate to these. We need to keep names that were originally proposed for largely reasonable groups and to follow the same system of restriction as we do for names in the Species-, Genus-, and Family-Groups.

The one escape mechanism is the " popularity contest " provision. With it, totally irrational groups can be ignored and, in general, prevailing usage maintained.

D. Coordination of Taxa

I admit that I have been critical of the idea of coordination of names of the Family-Group and that, to be consistent, I should also object to this principle at higher levels. The Botanists in their Code eschewed coordination from the very first—from the species level up. Concomitantly with this, however (and perhaps with wisdom) they have steadfastly refused to legislate the application of the Law of Priority for taxa above the level of order ; they have gone so far as to exclude such taxa explicitly from Priority. In many ways it is clear that the Botanists are twenty years ahead of the Zoologists in the perfection of their Code—although it is perhaps not fair to judge the two Codes in this way, for the plants strike me (on the basis of my recent studies of their evolutionary interrelationships) as posing less difficulties of classification than the animals. However, it may well be that, all this admitted, the Botanists have been more unified and clear-headed ; and their rejection of coordination strikes me as a good example of clear thinking. The fact is that they do have a better Code than we in Zoology. (In fact we have been essentially without a Code—in the sense of codification—since the revolutionary changes of 1948 at Paris, for, at that time and subsequently, much vital innovation has been made [as in the Copenhagen volume and also as published in the *Bulletin of Zoological Nomenclature* and in various of the *Opinions, Declarations* and *Directions* published by the International Commission in the series *Opinions and Declarations Rendered . . .*].)

I have done some soul-searching the last few weeks and especially the last few days, and have decided that, since the Principle of Coordination, as applied to species and genera, was a part of zoological nomenclature with the earliest official Code (1900) and since this concept has already been legislated for higher taxa as well, at Copenhagen, it perhaps serves best the interests of nomenclatural stability in zoology to accept this extension of the concept in question all the way up the hierarchy of taxa. So I am now prepared to accept it at the Family-Group level too. If, however, the Principle of Coordination is to be reversed at the Family-Group level, the same should be done at the Order/Class- and Phylum-Group levels as well. But you will, I hope, see that, in your Rule, you are asking for a reversal of this long standing zoological tradition.

E. Summary and Conclusions

I pointed out that I regard your Rule as contravening two basic principles of zoological nomenclature—that of the type concept and that of coordination. It is ironical that, at present, at least, you must look in the Code to the very ruling of which you appear to disapprove most strongly, for a source of support. As far as I can see, every other provision is designed to keep taxonomic (or systematic, if you prefer) ideas out of nomenclature as much as possible.

Frankly, I think that the most undesirable feature of your Rule lies in the fact that, at the same time that it demands radical departures from much past nomenclatural tradition, it relies basically on another nomenclatural tradition. Thus, on the one hand, it would, in effect: (1) circumvent the type concept (type genus in the case of families and higher taxa); and (2) at the same time, reject the time-honored process of exclusion, which has traditionally not affected the names of species, genera, and families, and whose application, as a result of the 1953 legislation, would appear to have been extended to higher taxa. On the other hand, your Rule rests squarely on the Law of Priority. In other words, you propose sweeping aside certain important traditions and yet at the same time requiring that the essentially new concepts be supported by the Law of Priority.

Well, all this is possible, but I doubt that it is desirable. Personally, I should prefer not to indulge in radical departure from current rules, but rather to work with them insofar as I can in good conscience. It is a strange predicament. On the one side I support the *Règles* as they are constituted, with full appreciation that the " popularity contest " provisions for higher taxa run counter to many of the traditional concepts of the earlier Code (but I accept the " popularity contest " provision because I realize that the *Règles* before 1953 did not have any explicit provisions for determining the names of higher taxa ; except a few for families and subordinate categories thereto. Thus the very promulgation of such rules was a radical departure ; and it does not disturb me if, in their promulgation, essentially new concepts are introduced). On the other side you would, in effect, replace the legislation of 1953 with concepts that are, in their way, as novel as the " popularity contest " provisions.

I am willing for the sake of our paper to go along, in the main body of the text, with your Rule and to express my demurrals in footnotes. But I hope that after reading and digesting what has been written in this appendix you will abandon some, at least, of your position. There are certainly many moral points to back you ; but, as I have said to you often, nomenclature as it has generally evolved has operated in certain vital respects independently of taxonomy and systematics. It has traditionally been a means of getting stable names—not of honoring people. The adding of author's names and dates has been (properly) regarded as an abbreviated bibliographic device. You are highly ambivalent on this matter—at one time you state that authors' names should be left off of higher taxa in order to discourage the incentive for personal glory ; another time you say that, for a given taxon, that name should be used that was applied to it at the time the group was first accurately characterized at the level accepted by you and that this is only right because it honors the person responsible (von Linstow and Cobb being two of your heroes in this connection]. I can only say that, to me, these are scarcely consistent viewpoints.

But I say what I said before—to me this is a non-Aristotelian world. Therefore, I do not object to multiple logical systems ; but I do like to know what I am doing and to be able to recognize where I am applying one set of logic and where another. I want you to do the same. Otherwise you will not be adequately prepared to meet the challenge of those whose systems of logic differ from yours.

APPENDIX III.

Conditions Causing Homonymy between Names of Higher Taxa

At the outset, let me explain what I believe the International Congress of Zoology means with respect to homonymy of names of higher taxa (see *Copenhagen Decisions* . . ., p. 42, Decision 68). In the Copenhagen volume it is stated that [in addition, by implication, to the fact that two names are homonyms when they are of identical spelling] two names that differ only in "termination" are also homonyms. You in effect raised the question of how this ruling is to be interpreted—in your letter of the 7th. Having been at Copenhagen, I can say that a lot of discussion went into the rulings later published as the *Copenhagen Decisions* . . . , but that in some cases the published version fails to do full justice to those discussions. In the particular case before us I can say that it is quite clear to me that what the Colloquium had in mind with respect to "terminations" were only the common neuter pleural adjectival endings *-a*, *-ea*, *-ia*, *-ida*, *-ina* (-oidea is a special case, which I discuss further on).

By contrast with the foregoing, when a compound word is made by combining the appropriate parts of two latinized Greek words (or two Latin words, or a Latin plus Greek or Greek plus Latin word in hybrid combination), a different word is formed, and the second part cannot be considered as a "termination" in the sense of the Copenhagen decision in question. Instead the stem (or root) of the compound word consists of both parts up to the declensional (usually adjectival) ending (*i.e.*, *-a*, *-ea*, etc.). [Since I first wrote the foregoing paragraph, it has become evident that you independently arrived at the point of view there expressed (*vide* P.S. to your letter of Nov. 9th).][2]

As an example, let us take the case exemplified on the one hand by the series that, in Pearse's system (1942), ran Spiruria-Spirurida-Spirurina. Here the difference lies in the *-ia*, *-ida*, *-ina* suffixes, which are merely adjectival in force and do not contribute any other meaning to the word than indication of the rank for the respective taxa. But, on the other hand, take the word Spiruromorphina, which I have suggested as a replacement for Spirurina: this consists of combinations of three Greek words, *σπεῖρα οὐρά*, and *μορφή*, plus the Latin adjectival ending *-ina*. To be more exact, the word is made up as follows—from:

(1) *σπεῖρα*→the stem *spir-* ;
(2) *οὐρά*→the stem *ur-* ;
(3) the conecting vowel -o- ;*

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[2] The letter here referred to was not furnished to the Office of the Commission by Dr. Dougherty.

* This is normal for Greek when two words are combined to give a compound word and the stem of the first one and the derivative of the second one begins with a consonant. For compound Latin words in the same situation the proper vowel is *-i-*. With hybrid (Greek-Latin or Latin-Greek) words the connecting vowel is *-o-* if the first part is Greek and *-i-* if the first part is Latin. (This last rule explains why *nematocide* [Greek-Latin] is to be preferred to *nematicide* [where the Latin combining vowel is used with a Greek stem]. Despite Cobb's contention, it did not usually make any difference, in the best Classic Latin, if a word had already been adopted from Greek; it still kept its "-o-" connecting vowel in hybrid compounds. A good classic example is *thermŏ-pōto* [< *θερμός*, *hot*; and pōto, *to serve drinks*—hence, *to refresh with hot drinks*].

(4) μορφπ→the stem *morph-* ;

(5) and, finally, the adjectival ending *-ina.*

Result : *Spir-ur-o-morph-ina.* The stem of this new compound word is *spiruromorph-*, and names based on the new stem *spiruromorph-* should pose no problems of homonymy with words based on the old stem *spirur-*. The contrary view, to my way of thinking, does violence to good linguistic sense.

I must admit, however, that the suffix *-oidea* is an embarrassment here. It is the neuter plural of Latin *-oideus*, *-oidea*, *oideum*, which in turn derives from the uncontracted Greek adjectival suffix -οειδης, which in its turn derives from the Greek word εῖδος (*form*). Viewed in this light, it might be held to have as much right to conferring independent status as should (I believe) *-morph*(*ina*). There is this difference, however : *-oidea* has been adopted in an adjectival sense for superfamilies and made homologous with the endings *-idae* and *-inae*, which apply, of course, to families and subfamilies respectively. (As indicated in an earlier letter [Nov. 4th—sent Nov. 7th],[3] *-idae* is a Latin noun suffix [masculine plural] and *-inae* a Latin adjectival suffix [feminine plural]). But I believe that the neo-Latin usages of all these endings have reduced them to the same function (adjectival in force) as those of higher categories and that, for purposes of determining homonymy, they should not usually count as parts of stems. I think that exception should be made, however, where the Greek word εῖδος is specifically given as the basis of word formation (just as I should propose in the case of μορφή).

No doubt the International Commission needs to go over these points and spell them out more clearly.

With respect to the ending *-acea* and your contention about it in your letter of the 5th, yes—I know that it is adjectival in force, being a Latin adjectival ending (like *-inae*, etc.) and that it has been used in Botany in the feminine [sic] plural form, *-aceae* [sic] (agreeing in number and gender with the kingdom Plantae), for families of plants. However, I doubt that it will find a place in any system of uniform zoological endings and therefore should prefer to leave it alone in the case of the word Gordicea. Your emendation Gordea does, it is true, preclude any possible future conflict in this respect. I should point out, however, that, in this sense, the stem of Gordiacea is *gordi-* and that Gordiea is a better emendation than Gordea. (Contraction of vowels was, of course, done in both Greek and Latin, but in the case of the neo-Latin names for the Family-Group, it has become the rule that the endings *-idae*, *-inae*, etc., should be added to the complete stem—thus the family GORDIIDAE.

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[3] The letter here referred to was not furnished to the Office of the Commission by Dr. Dougherty.

The same rule has been applied to the higher taxa of plants. It therefore would seem logical to follow this practice for names of higher zoological taxa—at least in making emendations where a stem of a higher taxon has already been established—as *gordi-* in Gordiacea.

My discussion of the status of compound words that include, as a suffix, a derivative of *μορφή* should, I feel, explain why I hold that it is improper to create a word Archescoleca or Archaeoscoleca and attribute it to Huxley. The name of Huxley's taxon Scolecida was derived only from *σκώληξ*; your alternative names are derived from *ἀρχε-* or *ἀρχαῖος* and *σκώληξ*. Linguistically they are hardly equivalent to Scolecida; nor can they reasonably be so nomenclatorially.

APPENDIX IV

A Summary of My Personal Views on Determining Names of Higher Taxa

1. General Principles

(1) I believe that the first consideration to be given to deciding the name of a higher taxon is universality and stability of usage; to me—as in the present *Règles*—this takes precedence over considerations of priority, etymology, etc. if it appears to me that a given name is important and most widely used as between two or more contenders, I don't give a hoot or holler whether it is historically etymologically, or philologically legitimate; where there is no overriding consideration of usage, however, I feel that priority is the best guide;

(2) I believe that, once a name is decided upon, its documentation should be determined: I hold that, as much as possible, this should be accomplished as an exercise of nomenclature, with minimal obtrusion of taxonomic concepts;

(3) I subscribe to the following further principles with respect to deciding the author and date of the name of a given higher taxon:

(a) the author of a higher taxon (*i.e.*, of the Order/Class- or Phylum-Group) is the first person to use the name at any level above the Family-Group (and for this purpose the adjectival group-endings can be ignored—it is the stem [or root] that counts); the date is that of first usage; it makes no difference whether the original grouping was unnatural, as long as it held a genus that would fit as a modern type genus of the taxon;

(b) where the original nominal group was taxonomically unnatural, I do not believe that, from the point of view of nomenclature, any recognition of the restricting author need be extended by analogy with the treatment of lower taxa (promotion or demotion of rank can, however, usefully be indicated by the device of parentheses; and changes in spelling [of the ending and, to a limited extent, of the stem] can be indicated by the device of square brackets);

(c) if, however, it were desired to indicate that some other than the original author were responsible for the taxonomic concept of a given nominal group, this could be indicated by using "*sensu*" followed by author and date.

2. The meaning of §, ♯, and ‡, of another symbol † and of the word "partim"

(1) By § I mean that a name, as originally used, contained groups not now in the group bearing the name, or, in the case of a synonym, not in the group with which it is synonymized; from the nomenclatural standpoint, however, I regard such groups as more or less co-extensive and as having the same type genus;

(2) by ♯ I mean that a name, as originally used, did not contain a group or groups known at the time the name was proposed and now included in the group bearing the name, or, in the case of a synonym, now in the group in which the synonym is listed; from the nomenclatural standpoint, I again regard such groups as more or less co-extensive and having the same type genus;

(3) by ‡ I mean that a name, as originally used, applied to a group now entirely included within, but comprising only a part of, the group with which it is synonymized; such groups have a type genus different from that of the group with which the included group is submerged, but the type genus of the included group is considered as not being separable, at least for the time being, from that of the including group;

(4) with "*partim*" I had wished to convey a quite different concept (I now feel I should abandon this proposal); what I had intended was that, where two or more groups were originally united under a name, but would at present be regarded as not belonging together and where no one had restricted the name to one of the natural group originally included and, furthermore, I should not wish to restrict it, I should list the name in the synonymy of each of the originally included groups, but qualify it in each case with "*partim*"; this was meant to indicate immediately that the name in question would be found in the synonymy of more than one group (you have, I believe, misunderstood this; the fault is, however, mine for not being clear); I now think that another symbol should be used and propose † (which, appropriately enough, also means dead);

(5) I now have decided that " *partim* " should be restricted to the sense in which it is usually applied in generic synonymies—*viz.*, to qualify a group name that, when first proposed, included that nominal group (or members of that group) in whose synonymy the name is being listed, but that, as now treated, does not or should not contain the former (or members of the former).

All these symbolic devices are designed to convey taxonomic concepts in what is otherwise a formalistic nomenclatural system.

3. Possible Modification and Amplification of Existing Rules

I have given what I consider the most reasonable analysis of the *Règles* as they apply to higher taxa. However, I am not strongly wedded to any one nomenclatural system. I can see some virtue in the argument that nomenclature and taxonomy should be brought somewhat more into line and that the rules for crediting authorship for higher taxa might be somewhat different from those used for families, genera, and species—or, what is implied more basically, that the choice of a name for a given taxon should be governed by considerations of systematic naturalness of the Group. But this, I feel, would be an exceedingly difficult thing to implement as an explicit provision of the *Règles*, requiring, as it would, a new departure in nomenclature, with, I am sure, unpredictable ramifications.

It may be admitted, however, that considerations such as the taxonomic naturalness have obviously played a decisive role in deciding in many zoological groups the names that are generally used. By consequence, such names will doubtless find their way on to the lists of names for zoological taxa such as are called for by the Copenhagen decision in this connection. Therefore, from this standpoint, the departure, mentioned in the immediately preceding paragraph, from past nomenclatural practices will, after all, play an inevitable role. But I believe that the Copenhagen provision that results in what you call a " popularity contest " handles this problem neatly and avoids a most difficult impasse—*viz.*, the obtrustion of taxonomic considerations into nomenclature to an extent that would greatly reduce nomenclatural stability.

I am sure that it is precisely because of this difficulty (which was perceived, but not exhaustively discussed) that the Copenhagen Congress on Zoological Nomenclature decided to have " lists " of names for the higher taxa of organisms prepared by " panels of specialists ". The issue of applying priority was specifically by-passed in this situation, although priority was declared binding in determining the status of names for higher categories created in the future. The effect of this is that experts in each group have a chance to express preference for a given name or set of names on any basis they desire. This the taxonomic problem tends to be divorced from the nomenclatural; for nomenclature would be fixed only after experts had decided the names in each group on whatever grounds they regarded as suitable.

A further effort to avoid, as much as possible, the intrusion of taxonomy into formal nomenclature was the Copenhagen decision to require type genera for higher taxa. Such type genera are meant to anchor names (*i.e.*, nomenclatural entities) into taxonomic systems.

In essence, the points that we have been arguing back and forth these past months are ones in which we have failed to agree on the relative roles of nomenclature and taxonomy (or systematics, if you will) in determining the choice of names.

I repeat—I hold that a nomenclatural system that depends as little as possible on taxonomic concepts—*i.e.* is " automatic "—is the best. Otherwise one is continually beset with problems of whether a given name should or should not be used because the taxonomic concept originally embraced by it is not that accepted today.

But what we have been arguing over is certain to reach the International Commission. We could no doubt serve a valuable function by organising our thinking, including our conflicting views, and presenting all for the Commission's consideration.

APPENDIX V

Application of the " Règles " to the names

Nemata *vs.* Nematoda, etc.

With the background of Appendices II–IV we can now conclude with a discussion of the relative status of Nemata and of Nematoda and its variants.

First of all, let us make clear the taxonomic (and systematic) problems. Both of us recognize that, in the nemas and horse-hair worms, we have two groups of independent phyla. We obviously need names for these taxonomic entities. So far there is, I am sure, no disagreement.

I believe we also agree on the essential historical facts—the earlier ones at least. Originally the name Nematoidea was given to an order containing both nemas and horse-hair worms—though primarily the former. This nominal order was, we feel, unnatural. Subsequently the Nematoidea (and various linguistic equivalents : Nematodes, Nematoda, etc.) fluctuated from the level of Order down to that of Family and up to that of Phylum, in accordance with the views of various investigators. Although the horse-hair worms were taken out of the nominal group Nematoidea by von Siebold in 1843, certain nemas (especially mermithids) were confusedly placed with the horse-hair worms until Vejdovský in 1886 clearly distinguished between the two groups and segregated the latter (as in the order Nematomorpha) from the former.

A point that we have never discussed is that Vajdovský, at the same time as he made a logical grouping for Nematomorpha, restricted the vernacular term " Nematoden " to an " Ordnung " for the nemas and thus created a completely natural nemic taxon. I do not know who was the first person to translate Vejdovský's concept into a formal neo-Latin word, but it is to be noted that Grobben (1909) had " Ordnungen " Nematodes and Nematomorpha in his Klasse Aschelminthes. So it seems clear that at least one variant of the word " Nematoda " was validly restricted to the nemas before Cobb created his phylum for them in 1919.

Now it appears that, under your Rule, the correct name for the Phylum of nemas would be Nemata. But what do the present *Règles* require ?

Insofar as they provide a guide, the following are the interpretations that appear to me to fit the *Règles* most closely :

(1) Nematoidea Rudolphi, 1808, is coordinate with all taxa of that name above the level of the Family-Group ;

(2) Nematoda Diesing, 1861, an orthographic variant of Nematoidea, is the name that would win, hands down, under the *Règles* " popularity contest " provision ; B. G. Chitwood is almost alone in favoring Nemata ;

(3) Nematoda as a taxon was validly restricted to the nemas—at least by Grobben (1910) and probably earlier ;

(4) in its promotion to phylar rank it must be reckoned as having maintained the same priority that it had at a lower level (see *Copenhagen Decisions* . . . , 1953, Decision 66 [pp. 41–42]) ; at the phylar level it therefore has priority over Nemata Cobb, 1919, even though it was promoted subsequently by Potts.

Now, I don't hold that all these rules are necessarily good ones. I tend to question, for example, that a promoted name should have priority over another name if the promotion was done after the latter was proposed. This is a point that the International Commission should, I think, re-examine. If they reverse themselves on it, then, of course, Pott's promotion of Nematoda would not affect Nemata Cobb, 1919.

However, we are still left with Lankester's phylum Nematoidea. By designation of a nemic genus as type, this would automatically become the name of the phylum of nemas under the Law of Priority even if coordination were done away with. To invalidate Lankester's group, the type system for higher taxa would have to be junked also.

In sort, I now conclude that I must point out in a demurring footnote in our paper that Nemata can only be validated either by changing the present *Règles* drastically, or by appealing for its preservation under the " popularity contest " provision.

My position is, as I have stated consistently, that Nematoda is the name of choice. If we accept this merely for the sake of argument, then how should it be documented ? Using the device of parentheses to allow indication of the person to promote it to the rank in which I accept it and of square brackets to fix responsibility for the spelling now used, one would have :

Nematoda ([Rudolphi, 1808] Diesing, 1861) Lankester, 1877.

Now, if it were to seem desirable to indicate also the sense in which this name is being used—*i.e.*, the person responsible for the taxonomic concept associated with the name, this could be indicated with the device of " *sensu* ". One should then have :

Phylum Nematoda ([Rudolphi, 1808] Diesing, 1861) Lankester, 1877, *sensu* Potts, 1932.

An even more extended version, which would indicate the fact that Lankester did not use the spelling Nematoda, would be :

Phylum Nematoda [([Rudolphi, 1808] Diesing, 1861) Lankester, 1877] Potts, 1932.

By this one would know that Rudolphi was responsible for the original word but did not spell it Nematoda, Diesing was the first one to use the present spelling, Lankester was the first to use it for a phylum, but not with the spelling Nematoda, and Potts was the first to use the spelling Nematoda at the phylar level. All of these are primarily nomenclatural facts and are not meant to document the historical sequence of taxonomic concepts that have been meant by Nematoda and its variants. The one taxonomic fact that would have to underly all, however, is that, in all its permutations, nomenclatural and taxonomic, Nematoda would be conceived as having the same type genus.

I realize that this leaves Cobb out, but to me the purpose of giving names and dates is, first and foremost, to document the nomenclatural facts. The taxonomic concepts are subordinate to these nomenclatural facts. It would, I believe, be incorrect to put Cobb's name in the foregoing series because his word Nemates (or, emended, Nemata) is of different classic origin and thus should be treated as nomenclaturally different from Nematoidea and its variants, including Nematoda.

Lists of author's names such as follow Nematoda in the foregoing examples would obviously not be used except in places where detailed nomenclatural documentation would be needed. In most cases, one could write merely the " Phylum Nematoda Rudolphi, 1808 " or possibly the " Phylum Nematoda Rudolphi, 1808, *sensu* Potts, 1932 ".

In finishing this difficult discussion, I might cite a few examples of the way in which I should apply the symbolism—§, ♯, ‡ and †. The examples can appropriately center around Nemata, etc. Since I have agreed that in the long paper your views should prevail in the text, I must assume first of all that the phylar name Nemata is to be used for the nemas. I believe that the main entry should be :

Phylum Nemata [Cobb, 1919] Pearse, 1936

The synonymy would be as follows :

† Intestina Linn., 1758 (Ordo-p. [n.v.]) [here † is used in the sense proposed in this letter ; " † Intestina " would also need to be listed in the synonymy of the subkingdom Amera.]

§ Nematoidea Rud., 1808 (Ordo—pp. 197, 198) [here the § means that organisms (*viz.*, certain horse-hair worms) were originally in Rudolphi's order, but we exclude them ; it also means, however, that the type of genus of Rudolphi's order is reckoned as the same as that of the phylum Nemata].

. . . [sundry names].

Gordiacea von Siebold, 1843 (Ordnung—pp. [362], 303), *partim* [here " *partim* " is used in the sense in which it is redefined earlier in this letter ; Gordiacea is included in this form in the synonymy of Nemata because it originally included certain nemas (mermithids), but no longer does].

† Nematalmia Vogt, 1851 (Klasse—pp. 174, 175) [here the † is used instead of " *partim* " in accordance with the revision proposed earlier in this letter].

♯ Nematoidei Vogt, 1851 (Ordnung—p. 181) [here the ♯ is used because Vogt's order did not include all nemas (*i.e.*, the mermithids were excluded)].

The foregoing examples should serve, I feel, to indicate how the symbols would function. I think, however, that using them for names in the Family-Group would be too complicated (at least at this time) for an enormous amount of checking would be required. I therefore propose that, if we are to use these symbols for the higher taxa, we nevertheless not use them for names in the Family-Group (except perhaps for ‡) and that this fact be specifically stated.

DOCUMENT 25/5

The relative merits of the Class names " Polyzoa " and " Bryozoa "

By **D. A. BROWN**

(*Senior Lecturer in Geology, University of Otago, Dunedin, New Zealand*)

(Letter dated 24th November 1953)[1]

As it seems likely, from my reading of recent numbers of the *Bulletin of Zoological Nomenclature*, that the Commission will soon make pronouncements on the naming of Taxonomic Categories above the Family level, I am prompted to make some observations in regard to the relative merits of the names Polyzoa and Bryozoa (Reference : Document 4/3, *Bull. zool. Nomencl.* **10** : 3). As an active worker on the group, I may say, from the outset that I firmly support the late Sir Sidney Harmer's preference for the term " Polyzoa " for reasons that will appear below.

First, there is no question as to the priority in time of J. Vaughan Thompson's " Polyzoa ". This point was fully dealt with by Harmer during the discussion on the two names by the Linnean Society of London in 1910 (*Proc. linn. Soc. Lond.* Session 123, esp. pp. 70–71).

Second, it is quite clear that Thompson recognized the Polyzoa as a distinct type of structure in the Animal Kingdom and his term " Polyzoa " may, therefore, quite fairly be used as that of a Class or Phylum.

In all the arguments over the relative merits of the terms Polyzoa and Bryozoa it has often been contended that because of the quaint wording employed by Thompson in his memoir (Zoological Researches, Memoir V, " On Polyzoa, a new animal discovered as an inhabitant of some Zoophites, with a description of the newly instituted Genera of *Pedicellaria* and *Vesicularia* ", December, 1830), the value of his term " Polyzoa " was never quite certain, being sometimes employed in the singular, sometimes in the plural, " Polyzoae ". In fact, one famous controversialist strongly suggested that Thompson regarded the term as of generic value, a contention immediately dispelled by the wording of the title to Thompson's paper which shows that it is, in fact, a group term.

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[1] This case has an associated Registered Number Z.N.(S.) 1310.

A close examination of the various arguments that were put forward at the aforementioned meeting of the Linnean Society of London in 1910, shows that although Thompson's Memoir was attacked vigorously on the grounds of bad syntax and grammatical construction, there was no denying the praise given to Thompson for the thoroughness of his researches and for his clear understanding of the group of animals that he was studying. It was not what he did but what he said that provided the proponents of the term " Bryozoa " with their chief arguments.

Oddly enough, it was never suggested that any critical examination be given to the work of the originator of the term " Bryozoa ", probably a mere coincidence. The term was first introduced by C. G. Ehrenberg in a portion of the " Symboliae Physicae " dated March 1831, a publication of much wider distribution and availability than Thompson's Memoir. (Incidentally, the late Sir Sidney Harmer in his application, *Bull. zool. Nomencl.* **1** : 230–231, was mistaken in thinking that the term " Bryozoa " did not occur in this work.)

Ehrenberg defined his Circulus Bryozoa in the following terms : " Ore anoque distinctis, tubo cibario perfecto. (Vibratio aperta ciliorum ope, an omnibus ? Ovipara et gemmipara, sponte nunquam dividua) ". This is scarcely a precise diagnosis, but then let us see what Ehrenberg ascribes to his Bryozoa. Not only what we call the Polyzoa but also a goodly portion of the Corals, the Sertularian Hydroids, and probably some of the other Coelenterata. So, although Ehrenberg's definition does give the more important characters of the Polyzoa, he had not, unlike Thompson, really discovered that they were unique. It is even more surprising to find that eight years later (*Phys. Abhandl. K. Akad. Wiss.*, Berlin (1838), pp. 59–120, 1839), when Ehrenberg brought out a fresh classification of the invertebrates, excluding insects, he included in his Order Bryozoa not only the groups mentioned above, but also added the Foraminifera. Thus, we may criticize Ehrenberg not only for what he did but also for what he said.

While it may be argued that, in general, the term " Bryozoa " has been employed more widely, geographically speaking, than the term " Polyzoa ", it is important to note that a large proportion of the work on the Phylum has been done by workers who have spoken of these animals as Polyzoa (Busk, Allman, Hincks, Norman, Kirkpatrick, MacGillivrary, Maplestone, Gray, Johnston, Lang, and in our own time, Miss Hastings and, most famous of all, the late Sir Sidney Harmer).

I believe that Vaughan Thompson's term " Polyzoa " should be universally adopted, not only because of its clear priority in time, but also as a tribute to a scientific worker of great merit. (See Harmer, *Bull. zool. Nomencl.* **1** : 230–231.) The obscurity of his published work should not be allowed to deprive him of his right to recognition.

DOCUMENT 25/6

Question of the name to be used for the Class typified by the genus " Chiton " Linnaeus, 1758

By **L. R. COX**

(*British Museum* (*Natural History*), *London*)

(Enclosure to a letter dated 23rd October 1956)[1]

It may be recalled that the nomenclature of higher taxonomic categories was discussed in 1953 at the Copenhagen Colloquium, and that it was decided that Committees of Specialists should be asked to make recommendations to the Commission regarding the names to be adopted for taxa belonging to the Order/Class-Group and to the Phylum-Group, it clearly not being the view of the Colloquium that acceptance of such names should be determined entirely by considerations of priority. No Committee of Specialists on the Mollusca is, however, at present in existence, and the formation and operation of such a Committee would appear likely to present considerable difficulties. The applicant has, therefore, decided to submit the present case direct to the Commission, making a definite recommendation in the hope that all workers with views on the subject will express them in writing for the guidance of members of the Commission.

2. There is much discrepancy in standard works of reference in the name applied to the Class* of the Phylum Mollusca which is typified by the genus *Chiton* Linné, 1758, and it is important that a decision should be reached as to which name shall receive official acceptance.

3. The following are the names which have been proposed for this Class.

Loricata C. F. Schumacher, 1817, *Essai d'un nouveau Système des habitations des vers testacés*, pp. 23, 35. Name applied to the " divisio secunda " of the " subsectio secunda " of the Monothalami, which name was applied

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* Or Order, in some systems of classification.

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[1] This case has an associated Registered Number Z.N.(S.) 1110.

to the " première section générale " of the " Vers testacés ". *Chiton* L. was the sole genus mentioned and no diagnosis of the Loricata was given.

Crepidopoda G. A. Goldfuss, 1820, *Handbuch der Zoologie*, Teil 1, pp. xliii, 624. Proposed as an Order of the Class Mollusca with *Chiton* L. as the sole included genus. The characters of the Order were described.

Polyplacophora J. E. Gray, 1821, *London Medical Repository*, Vol. 15, p. 234. Proposed, with a formal diagnosis, for an Order of the Class Gasteropodophora of the Sub-Kingdom Mollusca equivalent to the Linnean genus *Chiton*, three species of which were mentioned but included in what appear to be intended as new genera *Acanthochitona*, *Lepidochitona* and *Cryptoplax*.

Polyplaxiphora M. H. D. de Blainville, 1824, *Dict. Sci. nat.*, Vol. 32, p. 380. Regarded as a Class of the Mollusca co-extensive with the genus *Chiton* L.

Placophora H. von Ihering, 1876, *Jahrb. deutsch. malak. Ges.*, Jahrg. 3, p. 137. Proposed for a Class of the newly erected Phylum Amphineura, the Class being co-extensive with the family Chitonidae. A diagnosis was given.

Polyplaciphora W. H. Dall, 1878, *Proc. U.S. Nat. Mus.*, Vol. 1, p. 300, emendation of Polyplacophora.

Polybranchiata J. W. Spengel, 1881, *Zeits. wiss. Zool.*, Vol. 35, p. 356. Proposed for the chitons as an alternative name to Polyplacophora.

Lepidoglossa J. Thiele, 1893, in F. H. Troschel, *Das Gebiss der Schnecken*, Vol. 2, p. 353. Proposed for the chitons as an alternative name to Polyplacophora, referring to the characters of the radula.

The name Amphineura was proposed by H. von Ihering (1876, *Jahrb. deutsch. malak. Ges.*, Jahrg. 3, p. 136) for a Phylum founded to include the Classes Aplacophora (= families Chaetodermata and Nemeniadae) and Placophora, and excluded from the Mollusca. It was thus not synonymous with the series of names under consideration, and further reference need not be made to it.

4. Of the above names for the chitons, all except Loricata, Polyplacophora and Placophora have been generally disregarded and may be rejected without further discussion. Of the three names just mentioned, I list below those employed in (a) standard works of reference, (b) titles of a number of papers by modern authors :—

(a) In standard works of reference

P. Fischer, *Manual de Conchyliologie* (1880–7)	Polyplacophora
K. Zittel, *Handbuch der Palaeontologie* (1881–5)	Placophora
H. A. Pilsbry, Tryon's *Manual of Conchology* Vol. 14 (1892)	Polyplacophora
A. H. Cooke, " Molluscs " in *Cambridge Natural History* (1895)	Polyplacophora
H. A. Pilsbry, Eastman's edit. of Zittel's *Textbook of Palaeontology* (1900)	Polyplacophora
H. Simroth and H. Hoffman in Bronn, *Klassen und Ordnungen des Tierreichs* (1929)	Polyplacophora
T. J. Parker and W. A. Haswell, *Text Book of Zoology* (1940)	Placophora
J. Thiele, *Handbuch der Systematischen Weichtierkunde* (1929–31)	Loricata
R. Winckworth, The British Marine Mollusca, *Journ. Conch.* Vol. 19 (1932)	Loricata
C. Dechaseaux in Piveteau, *Traité de Paléontologie* (1952)	Polyplacophora
A. M. Jakovleva, *Tab. anal. Fauna URSS*, No. 45 (1952)	Loricata
S. Hirase and Isao Taki, *Illustrated Handbook of Shells* (1954)	Polyplacophora
Zoological Record for 1953 (1955)	Polyplacophora

(b) Titles of papers (taken from the " Zoological Record ")

Iredale and Hull (1927), Loricata ; Hull and Risbec (1931), Loricata ; V. Fretter (1937), Polyplacophora ; H. Leloup (1937–1952 numerous papers), Polyplacophora ; J. R. M. Bergenhayn (1930–2), Loricata ; J. R. M. Bergenhayn (1937 and 1946), Polyplacophora ; C. M. Yonge (1939), Loricata (Placophora) ; Cotton and Weeding (1940), Loricates (vernac.) ; L. Arvy and M. Gabe (1949), Polyplacophora ; M. Gabe and H. Prenant (1949), Polyplacophora ; Z. A. de Castellanos (1952), Polyplacophora ; P. Kaas (1953), Loricata ; J. R. M. Bergenhayn (1955), Loricata.

5. Although use of the name Loricata has increased in recent years, probably because of the importance which has been attached to priority, Polyplacophora still appears to be the most widely accepted name for the chitons. An objection to Loricata lies in the fact that this name has been also used both in Mammalia and Reptilia, although it should be mentioned that authorities who have been consulted do not favour its adoption in either of these groups.

6. In view of the foregoing considerations I now make application to the Commission :—

(1) To accept the name Polyplacophora as the valid name for the Molluscan Class typified by the genus *Chiton* Linné and to place it on the *Official List of Names in the Order/Class Group in Zoology.*

(2) To place the names Loricata, Crepidopoda, Polyplaxiphora, Placophora, Polyplaciphora, Polybranchiata and Lepidoglossa, for each of which the genus *Chiton* Linné, 1758, is here designated as type genus, on the *Official Index of Rejected and Invalid Names in the Order/Class Group in Zoology.*

(3) To place the generic name *Chiton* Linné, 1758, on the *Official List of Generic Names in Zoology.*

(4) To declare that the species *Chiton tuberculatus* Linné, 1758, shall be accepted as type species of the genus *Chiton* Linné, 1758, in accordance with the designation of W. H. Dall (1878, *Proc. U.S. nat. Mus.*, Vol. 1, p. 297), thereby setting aside any prior designation of any of the other three original Linnean species that may have been made.*

(5) To place the specific name *Chiton tuberculatus* Linné, 1758, on the *Official List of Specific Names in Zoology.*

(6) To place the family name CHITONIDAE C. F. Rafinesque, 1815 (published in the form CHITONIA and emended to CHITONIDAE by J. E. Gray, 1834) on the *Official List of Family-Group Names in Zoology.*

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* Dodge (H.), (1952), *Bull. Amer. Mus. nat. Hist.*, Vol. 100, p. 19, states that "in the Tenth Edition Linnaeus listed only four species in this genus : *hispidus, tuberculatus, aculeatus* and *punctatus.* Of the four only one, *tuberculatus*, has been identified ".

DOCUMENT 25/7

Order/Class Group Names in Zoology with special reference to the name to be used for the Order of Insects comprising the Fleas

By **G. H. E. HOPKINS**

(*British Museum (Natural History), The Zoological Museum, Tring, Herts., England*)

(Enclosure to a letter dated 8th August 1957)[1]

In 1953 (*Copenhagen Decisions on Zoological Nomenclature*, Decisions 56–69) provision was made for the first time concerning the methods to be used in determining what names shall be applied to taxa above the Family-Group level. Such names are to be fixed (" defined ") by selection of a type genus for each, and the mode of procedure proposed is that a Committee of Specialists in each major group of the Animal Kingdom should draw up lists of recommended names and of objectively invalid names within the field of their speciality ; in drawing up these lists first consideration is to be given to weight of usage, other considerations (such as priority) being set aside unless usage affords no clear basis for choice.

2. The decision that priority is not to be the main consideration in choice of names is most wise and necessary, but the procedure suggested seems to me to be hopelessly cumbersome, and the prospects of arriving at a conclusion by following it seem to be well indicated by the fact that no committees of specialists have been set up in the four years since it was decided upon. Moreover, an attempt to find out what names must be taken into account in the case of the fleas (Hopkins, 1951, *Entomologist* **84** : 208–214) has convinced me not only that there is a strong probability that there are many names in the Order/Class group which have been entirely overlooked ever since their publication, but that it is extremely undesirable that objective invalidity should be the only criterion for declaring names in this category to be

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[1] This case has an associated Registered Number Z.N.(S.) 1309.

unavailable. In the groups of insects on which I work the Order/Class group of names includes few, if any, objective synonyms because few, if any, selections of a type genus have been made, and it is most undesirable that any such selections should be made hastily lest unwanted names in one major group of animals should be much-wanted by workers in another field of zoology. Taking the fleas as an example, several names in the Order/Class group which have been applied to them also applied originally to other orders of insects, and to select as type genus of such taxa a genus of fleas would preclude their use by workers on any of these other orders.

It seems to me that a much simpler procedure would be more likely to be effective, and that, since my suggestions involve the substitution of an individual for the Committee of Specialists, it is essential that in the early stages any action taken by him must be provisional. I also think that the rejection of priority as the main criterion by which names are to be accepted or rejected makes it possible and desirable to set a time-limit in considering whether a given name is, or has been, in general use. My suggestions are as follows :—

(1) A single specialist in each major group of the Animal Kingdom should be invited (or may volunteer) to draw up, for the group of animals with which he is concerned, a list of names in the Order/Class-Group which are in general use, or have been in general use within the last 25 years, together with his recommendations as to which of them should be accepted (with his reasons for the recommendations) and as to the genus which should be selected as the type of each. These recommendations and selections of type genera to be provisional, having no validity until endorsed by the International Commission on Zoological Nomenclature.

(2) The list to be published in the *Bulletin of Zoological Nomenclature* to permit any interested specialist to put forward objections to any of the recommendations or suggestions for additions to the list of recommended names.

(3) After a suitable period, which I suggest might be six months, all proposals to which no objection has been made to be automatically accepted. All disputed proposals to be decided by the members of the International Commission of Zoological Nomenclature through the ordinary voting procedure. Names decided upon in this way to be dealt with as follows :—

(a) Accepted names to be placed on the *Official List* ;

(b) A declaration to be made that all other names in the Order/Class-group, which have been proposed, prior to the date of the declaration, for taxa within the group of the Animal Kingdom dealt with in the list concerned are to be regarded as invalid for all purposes within the group of the Animal Kingdom with which the list in question is concerned.

ANNEXE TO DOCUMENT 25/7

(Communicated to the Office of the Commission under cover of a letter dated 9th July 1957[2])

An annotated list of the Order/Class-group names which have been proposed for the fleas seems desirable, since its publication will afford an opportunity for workers on the group to inform me of their opinions as to names which should be adopted. A special complication is that the fleas form so homogeneous a group that they are now universally regarded as an order divisible directly into superfamilies without the intervention of suborders ; this raises the question whether names proposed for supposed suborders and based on characters now known to be fallacious ought to be preserved or abolished. Readers will find that the list differs considerably from that of Costa Lima and Hathaway (1946, *Pulgas*, p. 89) as regards some of the early references, particularly to works by French authors. This is because Costa Lima and Hathaway, who evidently did not have access to some of the rare books concerned, inevitably accepted the statements of later authors about these names ; the statements were, however, often incorrect, since many of these names were only put into Latin by the later authors and were originally published as vernacular words and have no validity (note, *e.g.*, that Lamarck, 1801, referred to Aptères and not Aptera). The list is arranged by date.

Aptera Linn., 1758, *Syst. Nat.* (ed. 10) 1, p. 608. First restricted to the fleas by Leach (1815, Brewster's *Edinburgh Encyclopaedia* (ed. 1) **9** : 76, 126). This restriction is much earlier than Shipley's action (1904, *Zool. Anz.* **27**, p. 260) in formally confining the name to another group of insects, but the name has been used extensively for the latter group and does not seem to have been employed for the fleas for at least a hundred years.

Saltatoria Retzius, 1783 and Suctoria Retzius, 1783, *Caroli . . . De Geer . . . Genera et Species Insectorum*, pp. iv, vi. " Subordo 4. *Saltatoria* " (p.iv) and " Classis 11. *Suctoria* " pp. iv, vi) both comprise only the fleas and have definitions reasonably diagnostic of the order. Saltatoria, which has page-precedence, has never been used again for the fleas but is in use for one of the main divisions of the Orthoptera. Suctoria is one of the three main claimants to be the correct name for the fleas.

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[2] For a note explaining the origin of the present paper see paragraph 7 in the Secretary's introductory note (Document 25/1) on page 493 of the present volume.

Rophoteira [Schellenberg & de Clairville], 1798, *Entomologie Helvetique* **1** : 44. The definition accompanying this name is by no means diagnostic but could apply to the fleas. It has never been employed for them, but Wagner (1939, *Aphaniptera*, p. 1) may be considered to have restricted it to them by placing it in the synonymy of Aphaniptera.

(Siphonata " Illiger, 1807 " Jordan, 1948, in Smart, *Insects of medical importance* (ed. 2), p. 211). There is no such name, merely a remark by Illiger " Hoc genus cel. Fabricio peculiarem ordinem praebebit, cui nomen *Siphonatorum* est. I.". The supposed name is probably a translation of the " Suceurs " of the early French writers, and is in Latin merely because the whole work is in that language. Siphonata has never been employed for the fleas, but Retzius used it in 1783 for the Homoptera. As applied to the fleas it dates from Jordan, 1948.

Medamoptera [Leach], [1815], *in* Brewster's *Edinburgh Encyclopaedia* (ed. 1) **9** : 76. Leach defines " Century II. Medamoptera " in a way that fits many insects besides the fleas, but it comprises only " Order X. Aptera ", with which it is, therefore, synonymous. The definition of Aptera given by Leach is also most unsatisfactory, but on p. 126 of Leach's work there is a definition which is diagnostic of the fleas and Suctoria Latreille is given as a synonym. The name Suctoria was apparently first published by Retzius in 1783 and is discussed above, but it was used by Latreille in 1805. Medamoptera has not been adopted by any subsequent author.

Aphaniptera Kirby and Spence, 1815, *Introduction to entomology* (ed. 1) **1** : 69. The date of this name has been variously given as 1816, 1818, 1822 and 1826, partly owing to a very natural confusion as to the actual date of publication (new editions of the first two volumes of Kirby and Spence's work were published before the first editions of vols. 3 and 4) and partly because of doubt as to whether it was anything but a *nomen nudum* before 1826. However, the statement " consisting of the flea genus " is apparently a sufficient definition according to the new rules, and this was published in 1815. The first definition was published in 1826 (*T.c.*, vol. 4, p. 367) where it is explained that the name is based on the belief (now known to be erroneous) that fleas have rudimentary or vestigial wings. The fact that the name perpetuates an erroneous belief is a main reason why many entomologists have refused to use it in spite of claims that it had priority. It is, however, one of the chief claimants to be the correct name of the order.

Siphonaptera Latreille, 1825 *Fam. nat. Règne anim.*, p.334. The name was published with a definition and the only complication is Latreille's claim that he had used the name earlier. If this was in a published work, the reference has never been traced. This name is one of the principal claimants be the correct name for the fleas.

Suctoridea Walker, 1851, *Insecta Britannica* **1** : 4. Used again by Walker in 1856 (*T.c.* **3**, p. 1) and apparently never afterwards.

Proboscidea Walker, 1851 and Eproboscidea Walker, 1851, *l.c.*, p. 4. These names were proposed for fleas with the legs " close side by side " and " distant " respectively. No examples are mentioned, the character is an illusory one, and it is impossible to suggest to what groups of fleas they were meant to apply. Nevertheless, the latter name is of some importance because it could be used (by anyone who may in the future consider the fleas to be divisible into suborders) on the grounds that names based on non-existent characters are less objectionable than the next senior subordinal names for the fleas, which are based on misleading characters. They have never been used again.

Integricipita Oudemans, 1908, and Fracticipita Oudemans, 1908, *Tijdschr. Ent.* **51** : 92. These names, like Proboscidea and Eprobiscidea, are in a different category from all those used previously, since they were proposed for suborders whereas all the previous names which applied to the fleas to the exclusion of other insects were employed for the whole order and are synonymical. Oudemans' two names are now disused, partly because no prominent worker on fleas now considers it desirable to divide them into suborders, and partly because they are based on a character now known to be of such trivial phylogenetic importance that instances occur in which the male of a species is fracticipit and the female integricipit. These four names seem to be the only ones which were applied to supposed suborders of fleas, certain others proposed by Oudemans which have the appearance of Order/Class group names (Solitothoracica and Brevithoracica Oudemans, 1908, Posttuberata, Intuberata, Longiclavata, Breviclavata, Dolichothoraca and Brevithoraca Oudemans, 1909) applying to Family-group taxa. Integricipita and Fracticipita were widely used at one time and were employed as recently as 1946 in Costa Lima and Hathaway's *Pulgas*, though it is not clear whether these authors accepted them. They are extremely objectionable because they convey the incorrect suggestion that the fracticipit or integricipit nature of the head is of fundamental importance.

Psyllomorpha Eysell, 1913, in Mense, *Handb. Tropenkr.* **1** : 81. Apparently never used by any other author.

Siphonata Jordan, 1948, in Smart, *Insects of medical importance* (ed. 2) : 211. This name has already been discussed under its erroneous attribution to Illiger, 1807.

It seems to me that the simplest and most satisfactory method of dealing with this problem would be for the International Commission on Zoological Nomenclature to declare either Siphonaptera or Aphaniptera (according to the wishes of the majority of specialists on fleas) to be the valid name of the Order and to declare all other names which have been proposed for the Order or for Suborders within it to be unavailable for this Order or for any taxon contained within it, thus not affecting their availability, if required, for any other group of the Animal Kingdom. It is not clear, however, whether this method would be permissible under the International Rules of Zoological Nomenclature at present in force.

DOCUMENT 25/8

Question of the Rules for the naming of Orders and taxa of higher rank

Views expressed by **HENNING LEMCHE**[1]

(*Universitetets Zoologiske Museum, Copenhagen*)

(a) Letter dated 13th July 1957 from **HENNING LEMCHE**

On certain occasions I have worked with problems concerning systematic matters of taxons higher than the Family-Group level. During such work, it is my experience that it is imperative to be able in some way or other to compare different views set forth by different authors, and to discuss their value. In all such cases, it is almost prohibitive to a lucid discussion if the same names are to be applied to different contents in different systematics, whereas discussion is easy if the names used in the conflicting views are all different.

For these reasons, I hereby make the formal proposal to the I.C.Z.N. that a paragraph be inserted in the Rules that

(1) Names of taxons of higher order than the Family-Group level are subject to the Rule of Priority only if they are used to designate the same complex of taxons of lower order.

Similarly, I ask that a *Recommandation* is adopted that

(2) In cases where a revision of some group necessitates the introduction of new and strongly deviating taxons of higher rank than the Family-Group level, authors are requested not to change the contents of the old names but to introduce new ones for their new ideas, in order that discussion of the relative value of the old and new systematic views may go on with as little confusion as possible.

The central thing is that, as long as we are on the specific or generic levels, a change of a name means that those acquainted with the old name is at a loss when confronted with a new one. On the Family-Group level, already, this is no more the case, since the family-name is based on a generic name which should be familiar—or at least identifiable—to the specialist. Names of higher categories stand exclusively for systematic ideas, and changes in their content deprive them of every value, as nobody will know what idea is behind the name in each single case when he meets it. Thus, stability obtained by giving priority to names of higher taxons means stability to empty names, whereas the ideas behind are left in the utmost confusion.

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[1] While the present Part was passing through the press, a further communication in regard to this problem was received from Dr. Lemche. This has been allotted the Document Number 25/10, and will be published in the next available Part of the present volume.

(b) Letter dated 19th July 1957 from **FRANCIS HEMMING** to **HENNING LEMCHE**

I agree with you that the Principle of Priority is one which should be applied to Order/Class names with the greatest care, even if it is to be applied at all. Your letter, however, places me in a difficult position because, under the Copenhagen Decisions, such names are not at present subject to priority at all. Accordingly, as it seems to me, your proposal, that names of this type should be subject to priority only in certain circumstances, represents a new departure in the sense that if adopted it would bring these names to some extent under the Law of Priority. I have a feeling that this is not the intention of your proposal, your idea being rather to limit them to extend the application of the Priority Principle at this level. Before I take any further action, I shall be grateful to receive your comments on the point raised above. I take it that I am right in concluding that you accept the view adopted by the Copenhagen Congress that no progress can be made with stabilising Order/Class names until type genera have been designated for the taxa in question.

(c) Letter dated 14th August 1957 from **HENNING LEMCHE**

As you correctly suppose it was never my intention to raise this question but only to safeguard the freedom in systematic work on higher levels. Hence, I should like to have my proposals regarded as subsidiary, only, i.e., to be used as soon as any proposal to introduce priority on these levels is appearing, and that my letter of July 13th should rest until such a situation arises.

Yes, I agree that no progress in stabilising these names can be made without indicating type genera, but even then it is difficult to see how stabilisation of these names could avoid being stabilisation, and hence paralyzation, of systematic work on higher levels.

DOCUMENT 25/9

Petition requesting clarification of the date and authorship of the Order/Class name " Monoplacophora "

By

J. BROOKES KNIGHT

(*Honorary Research Associate, Smithsonian Institution, Washington, D.C., U.S.A.*)

HENNING LEMCHE

(*Universitetets Zoologiske Museum, Copenhagen, Denmark*)

and

ELLIS L. YOCHELSON

(*United States Geological Survey, Washington, D.C., U.S.A.*)*

(Enclosure to a letter dated 16th September 1957)[1]

The Order/Class name Monoplacophora has come into usage for patelliform mollusks, the soft anatomy of which has not undergone torsion. Because of the phylogenetic importance of this group, the name will probably be cited frequently in the future and a prompt opinion by the International Commission of Zoological Nomenclature on the matter discussed below is requested.

2. The first usage of the name is in an article by Wenz, 1940 (*Arch. Moll.* **72** : 5) as follows : " . . . als sich bei einem Besuche N. Hj. Odhner herausstellte,

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* Publication authorized by the Director, U.S. Geological Survey, Washington 25, D.C.

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[1] This case has an associated Registered Number Z.N.(S.) 1258.

dass er zu einer ähnlicher Auffasung gekommen war und meinte, mann könnte die Tryblidiacea geradezu als Monoplacophora bezeichnen . . .". A free translation is as follows: " . . . during a visit N. Hj. Odhner stated that a similar concept had come to him, and frankly he thought one might designate the Tryblidiacea as Monoplacophora . . .". This is the only mention of Monoplacophora in the entire paper. There is neither description, diagnosis, or any other form of indication. The sentence quoted appears under a section headed " Die Tryblidiacea ". So far as it is known the name Monoplacophora was never used in any subsequent papers by Wenz or in any paper by Odhner.

3. The second usage of Monoplacophora is in a formal classification by Knight, 1952 (*Smiths. Misc. Coll.* **117** : 47) where the taxon is considered an order and diagnosed. Authorship of the term is attributed by Knight, 1952 (*Smiths. Misc. Coll.* **117** : 5) to Wenz. From the text it seems clear that Knight refers to the 1940 paper by Wenz. The third significant usage is a formal definition of Monoplacophora as a Class by Lemche, 1957 (*Nature* **179** : 413–414). Authorship of the taxon is attributed by Lemche to Odhner, 1940. Except for a mention of the name by Kaestner, 1952 (*Lehrb. Speziellen Zoologie*, Teil 1, Wirbellose (Lief. 2) : 229) so far as it is known, Monoplacophora has not been formally used in other publications.

4. It is clear that ambiguity surrounds the first usage of Monoplacophora and that the wording is subject to several different interpretations. We request that date and authorship of the taxon Monoplacophora be decided by the International Commission on Zoological Nomenclature in accordance with the *Copenhagen Decisions on Zoological Nomenclature*, Decision 114(3).

(1) The Commission is asked to choose among the three possibilities given below as to authorship and date of the Order/Class name Monoplacophora :

(a) Wenz, 1940

(b) Ohdner *in* Wenz, 1940

(c) Knight, 1952

(2) The Order/Class name Monoplacophora with approved authorship and date be placed on the *Official List of Order/Class-Group Names in Zoology.*

(3) Either (a) or (b) or both, depending on the decision of the Commission be placed on the *Official Index of Rejected and Invalid Order/Class-Group Names in Zoology.*

PURCHASED
31 MAR 1958

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The Official Organ of

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

Edited by

FRANCIS HEMMING, C.M.G., C.B.E.

Secretary to the International Commission on Zoological Nomenclature

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Professor Enrico TORTONESE (*Museo di Storia Naturale " G. Doria", Genova, Italy*) (16th December 1954)

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* Professor Esaki died on 14th December 1957, while the present Part was passing through the Press.

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CASE No. 26

DRAFT "RÈGLES": PROPOSED SCHEDULE GIVING GUIDANCE AS TO TRANSLITERATION OF WORDS FROM THE CYRILLIC ALPHABETS TO THE LATIN ALPHABET WHEN SUCH WORDS ARE USED AS ZOOLOGICAL NAMES

(Commission Reference: Z.N.(S.) 310)

DOCUMENT 26/1

On the problems involved in giving effect to the decision by the Thirteenth International Congress of Zoology, Paris, 1948, for the addition to the "Règles" of a Schedule giving guidance as to the transliteration into the Latin alphabet of words normally written in Cyrillic characters when such words are used as zoological names

By **FRANCIS HEMMING**, C.M.G., C.B.E.
(*Secretary to the International Commission on Zoological Nomenclature*)

(Report dated 1st February 1958)

The purpose of the present Report is to give a brief account of the action which has been taken for the purpose of drawing up a scheme for the transliteration into the Latin alphabet of words normally written with Cyrillic characters, when such words are used as zoological names. The immediate purpose of drawing up such a scheme is to provide the material needed by the forthcoming Fifteenth International Congress of Zoology at its meeting to be held in London in July of this year when it comes to consider the measures needed to give effect to the decision by the Thirteenth International Congress at Paris in 1948 that a Schedule giving such guidance be added to the *Règles* (see 1950, *Bull. zool. Nomencl.* **4**: 229).

2. At a very early stage it became apparent that the difficulties involved in giving effect to the foregoing project were much more formidable than they had appeared in Paris in 1948. Of these the most intractable is that there is no general agreement of an international character as to the principles which should be followed in transliterating words from Cyrillic characters into the Latin alphabet. Not only is this so, but in addition there are two sharply differentiated schools of thought as to the manner in which Cyrillic characters having no counterpart in the Latin alphabet should be transliterated, the first of these schools considering that such characters can best be transliterated by adding diacritic marks to letters properly belonging to the Latin alphabet, the second holding on the contrary that the better way is to indicate such characters by combining two or more letters of the Latin alphabet. Thus, if a Schedule of the kind contemplated is to be added to the *Règles*, it will be necessary to make a choice as to which of the above approaches to the subject is to be preferred, a matter on which it is too much to hope that complete agreement will be easy to secure.

3. Second, the problem is considerably broader in scope than was realised in Paris, for we are here concerned not with the transliteration of words belonging to a single language using an alphabet very different in many ways from the Latin alphabet, but with the transliteration of words belonging to Slav languages using no less than five different alphabets, each written in Cyrillic characters. If a generally acceptable basis could be devised for transliterating the Cyrillic characters employed in the Russian language, the difficulties involved by the foregoing complication would no doubt be capable of solution. It would, however, be necessary to take account of the fact that in some cases a particular letter does not have the same meaning in all the Slav languages and that in some cases the same letter is pronounced differently in different Slav languages. Moreover, a comprehensive scheme covering the characters in all the Slav languages would substantially increase the number of Latin-alphabet equivalents which would need to be found, the total number required in that event being forty-eight.

4. The problem involved in the present case was given a new turn by the decision by the Fourteenth International Congress of Zoology, Copenhagen, 1953, to ban the use of diacritic marks over letters in words when used as zoological names and to require that, where on the first publication of a zoological name a diacritic mark was attached to one of the letters included in the word of which that name was composed, the diacritic mark in question was to be replaced by a combination of letters to be prescribed in a Schedule to be attached to the *Règles* (1953, *Copenhagen Decisions zool. Nomencl.* : 57–58, Decision 101). The immediate effect of this decision was, as will be appreciated, to rule out of court all those systems for transliterating Cyrillic characters which rely upon the use of diacritic marks (paragraph 2 above). Accordingly,

the only system of transliterating Cyrillic characters that would be consistent with the foregoing decision by the Copenhagen Congress would be one by which the Cyrillic characters not possessing equivalents in the Latin alphabet would be rendered by specified combinations of letters belonging to the Latin alphabet.

5. At this point the consideration of the complicated and highly technical problems involved was greatly assisted by the receipt from Drs. Alexey Almasov and Estaban Boltovskoy (*Buenos Aires, Argentina*) of a communication discussing the present problem and submitting provisional proposals for a unified scheme of transliteration. These correspondents were at that time unaware that the Copenhagen Congress had banned the use of diacritic marks for zoological names and on their being notified of this decision it was necessary for them to modify their scheme in respect of the one case where they had recommended the use of a diacritic mark over a letter of the Latin alphabet to denote a particular Cyrillic character. On receipt of the foregoing minor amendment it was decided to put forward the scheme submitted by Drs. Almasov and Boltovskoy as the basis for the discussion of the problems raised by the Paris decision of 1948. That Plan was accordingly published in the *Bulletin of Zoological Nomenclature* on 31st January 1955 (Almasov (A.) & Boltovskoy (E.), 1955, *Bull. zool. Nomencl.* **11** : 7–18, 2 figs.). This fundamental contribution to the consideration of the present subject is being reprinted for inclusion in the London Agenda Paper where it appears as Document 26/2.

6. The plan prepared by Drs. Almasov and Boltovskoy was preceded by a note prepared by myself as Secretary (Hemming, 1955, *Bull. zool. Nomencl.* **11** : 4–7) in which I appealed to specialists to furnish comments on the scheme thus thrown open for discussion. In view of the peculiar nature of the present problem, the interest of which extended far outside the field of zoological literature, it was decided that steps of an altogether exceptional character should be taken by means of direct approaches to seek the views not only of leading zoological institutions but also of institutions concerned with philology, of great libraries on whose work also the present problem impinges, and also of leading specialists likely to be interested. In pursuance of this decision four hundred copies of the Plan drawn up by Drs. Almasov and Boltovskoy were made available to the Office of the Commission by the International Trust for Zoological Nomenclature. Part of these supplies was distributed direct from the Office of the Commission, while the remainder was transmitted to the authors of the Plan, who made themselves responsible for the distribution of the copies so supplied to them. In view of the paramount interest of this question to institutions and specialists in the U.S.S.R. and other countries using Slav tongues, the list of institutions to be consulted in those countries was drawn up on an extremely comprehensive basis, the number of letters so issued amounting to between eighty and ninety. The text of the letter issued in these cases, together with particulars of the Institutions so consulted, is shown in the Appendix attached to the present Report.

7. When at the time of its publication in 1955 the Plan prepared by Drs. Almasov and Boltovskoy was thrown open for discussion by being published in the *Bulletin of Zoological Nomenclature* it was arranged that comments on that Plan might be sent either to the Office of the Commission or direct to Dr. Almasov, to whom also copies of comments received by the Office of the Commission would be forwarded for information. It was then contemplated that at the close of the investigation a comprehensive Report would be prepared by Dr. Almasov covering the comments received from all sources. Unfortunately, circumstances have prevented this arrangement from being carried into effect. In consequence, it is possible now only to present those comments which were addressed to the Office of the Commission direct.

8. In all nine communications were received in regard to the present matter ; in addition, consideration was given to the Scheme in respect of certain of the Slav languages drawn up by the Royal Society in 1953 for use for bibliographical purposes. Four of the specialists who submitted comments expressed varying degrees of interest in, and support for, the principles underlying the Almasov/Boltovskoy Plan but indicated disagreement with certain of its provisions. The specialists concerned were : (1) Professor Dr. E. M. Hering (*Zoologisches Museum der Humboldt Universität zu Berlin*) (Document 26/3) ; (2) Dr. G. Witenberg (*Department of Parasitology, The Hebrew University, Jerusalem, Israel*) (Document 26/4) ; (3) Professor Dr. Tadeusz Jaczewski (*Institute of Zoology, Polish Academy of Sciences, Warsaw, Poland*) (Document 26/6) ; (4) Dr. Leo Sheljuzhko (*Zoologische Sammlung des Bayerischen Staates, Entomologische Abteilung, München, Germany*) (Document 26/9). Another zoologist, Dr. Curtis W. Sabrosky (*United States Department of Agriculture, Agricultural Research Service, Entomology Research Branch, Washington, D.C., U.S.A.*), while not commenting on the technical issues involved in the Plan, expressed support for the principle that the use of diacritic marks should be avoided which was embodied in it (Document 26/5). Another zoologist, Dr. Joshua L. Baily, Jr. (*San Diego, California, U.S.A.*) took issue with the authors of the Plan on their adoption of an orthographic, rather than a phonetic, basis for their scheme (Document 26/7). Dr. H. S. Bushell (*Commonwealth Institute of Entomology, London*), after enumerating the four conditions which a transliteration plan should satisfy, expressed the view that one only of those conditions was met by the Almasov/Boltovskoy Plan ; Dr. Bushell commented particularly upon the difference in meaning attaching to certain Cyrillic characters in various Slav countries and referred to the consultations undertaken by the Royal Society when it drew up its plan for " The Transliteration of Russian, Serbian and Bulgarian for bibliographical purposes " (Documents 26/12 and 26/13) ; he believed that it was a weakness in the Almasov/Boltovskoy Plan that it attempted to provide for the needs not only of zoological nomenclature but also for those of bibliography and was of the opinion that it should be possible to devise a simpler and more satisfactory scheme, if its purpose were to be strictly confined to zoological nomenclature

(Document 26/10). In November 1956 two letters on this subject were received from P. J. M. Geelan who, after drawing attention to the schemes for the transliteration of Russian and Bulgarian Place Names drawn up by the Permanent Committee on Geographical Names for British Official Use (Document 26/14), added the personal opinion that the best course would be for the International Congress of Zoology to adopt one or other of the national transliteration systems for use in zoological nomenclature (Document 26/11).

9. Finally, and, in my opinion, of outstanding importance in the present connection is a letter dated 5th September 1955 which was received from Dr. D. M. Steinberg, Vice-Director of the Zoological Institute of the Academy of Sciences of the U.S.S.R., covering a statement signed by Dr. A. A. Reformatsky on behalf of the Institute setting out the opinion of the Bureau of the Section of General and Comparative Linguistics of the Institute of Linguistics of the Academy of Sciences of the U.S.S.R. (Document 26/8). In this latter document objection was raised to the Almasov/Boltovskoy Plan and a method of transliteration involving an extensive use of diacritic marks was advocated. In the covering letter from the Institute of Zoology the foregoing Plan was examined not from a purely linguistic point of view but from the standpoint of zoological nomenclature. As regards this, the Institute of Zoology found the Plan, so far as it was related to the Russian alphabet " quite acceptable with the exception of the letter No. 39 (Ъ), which we think more desirable to succeed by the sign ("), when used in the middle of words, and omit when used at the end of words ". As regards letters used not only in the Russian alphabet, but also in the Ukrainian and Belorussian languages, the Institute of Zoology expressed the view that it was desirable, if possible, that the views should be sought of the Academies of Sciences of the Ukrainian and Belorussian S.S.R. respectively.

10. As regards the point raised in the second part of the reply received from the Institute of Zoology of the Academy of Sciences of the U.S.S.R., it must be recalled at this point that, as shown in the Appendix to the present paper, the Academies of Sciences of the Ukrainian and Belorussian S.S.R. were invited to submit observations on the Almasov/Boltovskoy Plan in April 1955 at the same time that similar invitations were issued to the Academy of Sciences of the U.S.S.R. and to the Academies of Sciences of other Republics in the Soviet Union. No replies were, however, received either from the Academy of Sciences of the Ukrainian S.S.R. or from the Academy of Sciences of the Belorussian S.S.R.

11. Looking at the comments received as a whole, it seems reasonable to conclude :—

(a) that the various systems (such as those discussed in paragraph 8 above), e.g., in Documents 26/12 and 26/14 which involve at least some use of diacritic marks are unsuitable for use for zoological nomenclature ;

(b) that the system which is required for zoological nomenclature is one based on the principles embodied in the Almasov/Boltovskoy Plan under which the Cyrillic characters which have no direct equivalent in the Latin alphabet would be rendered in that language (as used for zoological nomenclature) by combinations of letters, the use of diacritic marks being avoided ;

(c) that, as pointed out in a number of the documents submitted (e.g., in Documents 26/6 and 26/11), it would be undesirable to adopt for zoological nomenclature any system, however technically ingenious, that represented the views of individuals only and that what is required is a scheme which will both be suitable for zoological nomenclature (by reason of the avoidance of diacritic marks) and will command the support of important national bodies concerned generally with the present problem ;

(d) that, having regard to the fact that the subject under consideration is the transliteration of characters from the Cyrillic alphabets, particular weight should be given to the views expressed by Institutions in the U.S.S.R. and other countries, the mother tongue of which is written in Cyrillic characters ;

(e) that the communication received from the Institute of Zoology of the Academy of Sciences of the U.S.S.R. (Document 26/8) justifies the conclusion that, so far as the Russian alphabet is concerned, the Almasov/Boltovskoy Plan, would be acceptable to zoologists in the U.S.S.R., subject to the adoption of the suggestion made by the Institute of Zoology in regard to Letter No. 39 ;

(f) that, as the Academies of Sciences of the Ukrainian and Belorussian S.S.R. did not respond to the invitation that they should comment on the Almasov/Boltovskoy Plan, it may be assumed that no objection is felt by those bodies towards those parts of that Plan which specially concerns the transliteration of words belonging to the Ukrainian and Belorussian languages respectively.

12. In the circumstances the following propositions are submitted for consideration :—

(1) that, subject to the amendment as regards Letter No. 39 recommended by the Institute of Zoology of the Academy of Sciences of the U.S.S.R., the Almasov/Boltovskoy Plan relating to the transliteration of Cyrillic characters into the Latin alphabet be approved for the purposes of zoological nomenclature ;

(2) that a Section embodying the foregoing portion of the Plan be inserted in the First Schedule to the *Règles* as the method which zoologists are recommended to follow when transliterating from Cyrillic characters to letters of the Latin alphabet words intended to be used as Latinised zoological names.

APPENDIX TO DOCUMENT 26/1

(a) Copy of a letter despatched by the Office of the Commission on 2nd February 1955 to certain institutions in the U.S.S.R. and in other countries using Slav languages

Transliteration of words normally written in Cyrillic characters for the purpose of forming zoological names

On behalf of the International Commission on Zoological Nomenclature I write to seek the views of your Institution on the question of the method to be adopted for transliterating words normally written in Cyrillic characters for the purpose of forming zoological names.

In this connection I have to refer to two decisions taken by the International Congress of Zoology which bear upon this matter. First, the Thirteenth International Congress of Zoology, Paris, 1948, decided to attach to the *Règles Internationales de la Nomenclature Zoologique* a Schedule giving advice as to the manner in which words normally written in Cyrillic characters should be transliterated for the purposes of zoological nomenclature. Second, the Fourteenth International Congress of Zoology, Copenhagen, 1953, decided (a) to abolish the use of diacritic marks over letters of words when used as zoological names, and (b) for the purpose of forming zoological names to prescribe methods for indicating by means of the addition of a supplementary letter, a letter which would otherwise have borne a diacritic mark, for example by adding the letter " e " to denote, in the case of a German word a modified letter " u " in place of using an umlaut.

In view of the decisions indicated above, the system of transliteration to be embodied in the *Règles Internationales* for the formation of zoological names based upon words derived from languages using one or other of the

Cyrillic alphabets will necessarily differ in certain respects from any of the transliteration systems hitherto devised.

In order to make a start with the study of this important problem, the International Trust for Zoological Nomenclature has published in its *Bulletin of Zoological Nomenclature* a paper by MM. Alexey Almasov and Esteban Boltovskoy containing proposals for a scheme of transliteration for use when forming zoological names. It will be noted that this scheme covers five alphabets using Cyrillic characters and that in consequence forty-eight letters are involved.

The International Commission is anxious to devise the best system of transliteration obtainable within the limits laid down by General Directive issued to it by the International Congress of Zoology banning the use of diacritic marks. For this purpose special arrangements have been made for an exceptionally wide canvass of opinion among both zoological and philological institutions as a preliminary to the taking by the Commission of a final decision in this matter.

At the request of the International Commission I enclose herewith for the consideration of your Institution a copy of the paper containing the plan drawn up by MM. Almasov and Boltovskoy. It is the hope of the International Commission that your Institution will co-operate in this important enterprise by furnishing comments on the foregoing plan or otherwise.

Comments prepared in response to the present invitation should be addressed to Francis Hemming at the address shown at the head of the present letter. It would be a great convenience if such comments could be furnished in duplicate. Comments should, if possible, reach this office not later than 15th October 1955.

(b) List of Institutions in the U.S.S.R. and other countries speaking Slav languages to which the Almasov/Boltovskoy Plan was communicated for observations on 2nd February 1955 (see (a) above)

(i) U.S.S.R.

1. Academy of Sciences of the U.S.S.R.,
   B. Kaluzhskaja 14, Moscow, U.S.S.R.
2. Academy of Sciences of the U.S.S.R.,
   Department of Biological Sciences, B. Kaluzhskaja 14, Moscow, U.S.S.R.

3. Academy of Sciences of the U.S.S.R.,
   Department of Literature and Language,
   Volkhonka 18/2, Moscow, U.S.S.R.

4. Academy of Sciences of the U.S.S.R.,
   Institute of Language and Thought,
   Universitetskaja Naberezhnaia 5, Leningrad, U.S.S.R.

5. Academy of Sciences of the Armenian S.S.R.,
   Department of Social Sciences, Erevan, U.S.S.R.

6. Academy of Sciences of the Azerbajdzhanian S S.R.,
   Department of Social Sciences, Baku, U.S.S.R.

7. Academy of Sciences of the Belorussian S.S.R.,
   Department of Social Sciences, Minsk, U.S.S.R.

8. Academy of Sciences of the Estonian S.S.R.,
   Department of Social Sciences, Tallin, U.S.S.R.

9. Academy of Sciences of the Georgian S.S.R.,
   Department of Social Sciences, Tbilisi, U.S.S.R.

10. Academy of Sciences of the Kazakh S.S.R.,
    Department of Social Sciences, Alma Ata, U.S.S.R.

11. Academy of Sciences of the Latvian S.S.R.,
    Department of Social Sciences, Riga, U.S.S.R.

12. Academy of Sciences of the Lithuanian S.S.R.,
    Department of Social Sciences, Vilnius, U.S.S.R.

13. Academy of Sciences of the Tadzhik S.S.R.,
    Department of Social Sciences, Stalinabad, U.S.S.R.

14. Academy of Sciences of the Turkmenian S.S.R.,
    Department of Social Sciences, Ashkabad, U.S.S.R.

15. Academy of Sciences of the Ukrainian S.S.R.,
    Department of Social Sciences, Kiev, U.S.S.R.

16. Academy of Sciences of the Uzbek, S.S.S.R.,
    Department of Social Sciences, Tashkent, U.S.S.R.

17. Library of the Academy of Sciences of the U.S.S.R.,
    Birzhevaja linija 1, Leningrad, U.S.S.R.

18. State Public Library,
    Nevskij Prospekt and ul. 3-go Ijulja, Leningrad, U.S.S.R.

19. Lenin State Library,
    Mokhovaja 3, Moscow, U.S.S.R.

20. State Library of Foreign Literature,
    Stoleshnikov per. 2, Moscow, U.S.S.R.

21. Belorussian State Library,
    Minsk, U.S.S.R.

22. Korolenko State Library,
    Khar'kov, U.S.S.R.

23. Moscow State University, Faculty of Biology, Chair of General Zoology,
    Mokhovaja 11, Moscow, U.S.S.R.

24. Moscow State University, Faculty of Philology,
    Mokhovaja 11, Moscow, U.S.S.R.

25. Leningrad State University, Faculty of Biology, Chair of General Zoology,
    Universitetskaja nab. 7/9, Leningrad, U.S.S.R.

26. Leningrad State University, Faculty of Philology, Chair of Slav Languages,
    Universitetskaja nab. 11, Leningrad, U.S.S.R.

27. Leningrad State University, Faculty of Philology, Chair of Romano-Germanistic Languages,
    Universitetskaja nab. 11, Leningrad, U.S.S.R.

28. Leningrad State University, Faculty of Philology, Chair of West European Literatures,
    Universitetskaja nab. 9, Leningrad, U.S.S.R.

29. Belorussian State University, Faculty of Philology,
    University City, Minsk, U.S.S.R.

30. Belorussian State University, Faculty of Biology, Chair of General Zoology,
    University City, Minsk, U.S.S.R.

31. Central Asian State University, Faculty of Philology,
    Ul. K. Markas 35, Tashkent, U.S.S.R.

32. Central Asian State University, Faculty of Biology, Chair of General Zoology,
    Ul. K. Markas 35, Tashkent, U.S.S.R.

33. Kaunas State University, Faculty of Philology,
    Kaunas, U.S.S.R.

34. Kaunas State University, Faculty of Biology, Chair of General Zoology,
Kaunas, U.S.S.R.

35. Kazan' State University, Faculty of Philology,
Ul. Chernyshevskogo 18, Kazan', U.S.S.R.

36. Kiev State University, Faculty of Philology,
Vladimirskaja 58, Kiev, U.S.S.R.

37. Kiev State University, Faculty of Biology, Chair of General Zoology,
Vladimirskaja 58, Kiev, U.S.S.R.

38. Kazan' University, Faculty of Biology, Chair of General Zoology,
Ul. Chernyshevskogo 18, Kazan', U.S.S.R.

39. Chernovithy State University,
Universitetskaja 16, Chernovithy, U.S.S.R.

40. Latvian State University,
Bul'var Rainisa 9, Riga, U.S.S.R.

41. Lvov State University,
Marshalovskaja 1, Lvov, U.S.S.R.

42. Tartu State University,
Tartu, U.S.S.R.

43. Vilnius State University,
Vilnius, U.S.S.R.

44. Odessa State University, Faculty of Philology,
Ul. Petra Velikogo, Odessa, U.S.S.R.

45. Odessa State University, Faculty of Biology, Chair of General Zoology,
Ul. Petra Velikogo, Odessa, U.S.S.R.

46. Uzbek State University,
Bul'var Gor'kogo 15, Samarkand, U.S.S.R.

47. Uzhgorod State University,
Pl. Gor'kogo 1/3, Uzhgorod, U.S.S.R.

48. Voronezh State University,
Prospekt Revoluthii 24, Voronezh, U.S.S.R.

49. Khar'kov State University,
Ul. Svobodnoj Akademii 16, Khar'kov, U.S.S.R.

50. Gor'kij State University,
Sovethkaja Pl. 8, Gor'kij, U.S.S.R.

51. Academy of Sciences of the Armenian S.S.R.,
Department of Biological Sciences, Erevan, U.S.S.R.

52. Academy of Sciences of the Azerbajdzhanian S.S.R.,
Department of Biological Sciences, Baku, U.S.S.R.

53. Academy of Sciences of the Belorussian S.S.R.,
Department of Biological Sciences, Minsk, U.S.S.R.

54. Academy of Sciences of the Estonian S.S.R.,
Department of Biological Sciences, Tallin, U.S.S.R.

55. Academy of Sciences of the Georgian S.S.R.,
Department of Biological Sciences, Tbilisi, U.S.S.R.

56. Academy of Sciences of the Kazakh S.S.R.,
Department of Biological Sciences, Alma Ata, U.S.S.R.

57. Academy of Sciences of the Latvian S.S.R.,
Department of Biological Sciences, Riga, U.S.S.R.

58. Academy of Sciences of the Lithuanian S.S.R.,
Department of Biological Sciences, Vilnius, U.S.S.R.

59. Academy of Sciences of the Tadzhik, S.S.R.,
Department of Biological Sciences, Stalinabad, U.S.S.R.

60. Academy of Sciences of the Turkmenian S.S.R.,
Department of Biological Sciences, Ashkhabad, U.S.S.R.

61. Academy of Sciences of the Ukrainian S.S.R.,
Department of Biological Sciences, Kiev, U.S.S.R.

62. Academy of Sciences of the Uzbek, S.S.S.R.,
Department of Biological Sciences, Tashkent, U.S.S.R.

(ii) Poland

63. Polish Academy of Sciences, Section of Social Sciences,
Warsaw, Poland.

64. Polish Academy of Sciences, Section of Biological Sciences,
Warsaw, Poland.

65. Library of Jagellonian University,
    Al. Mickiewicza 22, Cracow, Poland.

66. Library of Warsaw University,
    Krakowskie Przedmescie 26-28, Warsaw, Poland.

67. National Library,
    Rakowiecka 6, Warsaw, Poland.

68. Uniwersytet Warszawski,
    Krakowskie Przedmescie 26-28, Warsaw, Poland.

69. Unywersytet Lodzki, Lodz, Poland.

70. Uniwersytet Poznanski, Poznan, Poland.

(iii) **Bulgaria**

71. Bulgarian Academy of Sciences, Section of Biological Sciences,
    7th November Street, Sofia, Bulgaria.

72. Bulgarian Academy of Sciences, Section of Social Sciences,
    7th November Street, Sofia, Bulgaria.

73. Central Library,
    7th November Street, Sofia, Bulgaria.

74. " Vassil Kolarov " State Library,
    Boulevard Tolbukhin 11, Sofia, Bulgaria.

75. Sofijski Universitet, Sofia, Bulgaria.

(iv) **Yugoslavia**

76. Srpska Akademija Nauka,
    Knez Mihailova ulica 35, Belgrad, Yugoslavia.

77. Slovenska Akademija znanosti in umetnosti,
    Postni predal 323, Ljubljana, Yugoslavia.

78. People Library,
    Knez Mihailova 56, Belgrad, Yugoslavia.

79. Narodna in universitetna knjiznica,
    Ljubljana, Yugoslavia.

80. University of Belgrad,
    Belgrad, Yugoslavia.

81. University of Ljubljana,
    Ljubljana, Yugoslavia.

(v) **Czechoslovakia**

82. Czechoslovak Academy of Sciences,
Narodni Tr. 5, Prague 1, Czechoslovakia.

83. Czechoslovak Standards Bureau,
Vaclavske nam. 19, Prague, Czechoslovakia.

84. Czechoslovak Academy of Sciences Fundamental Library,
Narodni Tr. 5, Prague 1, Czechoslovakia.

85. State Comenius Library,
Mikulandska, Prague, Czechoslovakia.

86. Charles University, Faculty of Philosophy,
Parizska tr. 27, Prague, Czechoslovakia.

87. Masaryk University,
Brno, Czechoslovakia.

88. Slovak University of Bratislava,
Ul. Paulinyho 1, Bratislava, Czechoslovakia.

DOCUMENT 26/2

A Plan for the treatment of words written with Cyrillic characters for the purposes of zoological nomenclature published in 1955 as a basis for discussion in relation to the Schedule giving guidance in the above matter, the addition of which to the " Règles " was agreed upon, in principle, by the Thirteenth International Congress of Zoology, Paris, 1948

By **ALEXEY ALMASOV** and **ESTEBAN BOLTOVSKOY**

(*Buenos Aires, Argentina*)

[Editorial Note : The present paper was published with the title " On the Treatment of Words written with Cyrillic Characters, for the purposes of Zoological Nomenclature, Bibliography, Reference Indices, Etc." on 31st January 1955 (*Bull. zool. Nomencl.* 11 : 7–18)]

Several works on zoology have raised lately the problem of transcription of words from languages using the Cyrillic alphabets. The aim of the present paper is to analyse the difficulties which confront scientists and those who are working in libraries and publishing houses and have to face the chaos existing in this domain. We attempt to show the defects of transliteration methods now in use and propose a new system which could be applied equally to all the five principle languages using Cyrillic alphabets, and would thus contribute to the establishment of a scientific nomenclature. Our proposals are set out in the table in the present paper.

2. Strange as it may seem, there is at present no unified and generally accepted transliteration system from the Cyrillic alphabets. On the other hand, all the individual systems in use today have, from the point of view of zoological nomenclature, one major defect, caused by their authors having set out from the phonetic relationship between the various Slav languages and their mother tongues. Thus, a single Russian river figures as " Tschir " in a German work, whilst an Englishman refers to it as " Chir.". There is therefore no need to emphasise the difficulties confronting a scientist about to compile a reference index on the basis of scientific literature in the various languages read by him. Furthermore, even translators into one and the same language are often guided by their own taste in transliterating names, so that one English work cites the name of a Russian town as " Eysk ", whereas

No.	Cyrillic letters	Proposed Latin alphabet equivalent	No.	Cyrillic letters	Proposed Latin alphabet equivalent
1	А а	*a*	13	И и	*i*
2	Б б	*b*	14	І і	*ih*
3	В в	*v*	15	Ї ї	*ji*
4	Г г	*g*	16	Й й	*j*
5	Ґ ґ	*gh*	17	Ј ј	*j*
6	Д д	*d*	18	К к	*k*
7	Ђ ђ	*dj*	19	Л л	*l*
8	Е е	*e*	20	Љ љ	*lj*
9	Є є	*jeh*	21	М м	*m*
10	Ё ё	*ew*	22	Н н	*n*
11	Ж ж	*zh*	23	Њ њ	*nj*
12	З з	*z*	24	О о	*o*

Figures 1 and 2 : Plan for the transliteration into the Latin alphabet of words normally written that the above (or any other) system should be mandatory. What is proposed is that an approved forming names based on words normally written in Cyrillic characters).

No.	Cyrillic letters		Proposed Latin alphabet equivalent	No.	Cyrillic letters		Proposed Latin alphabet equivalent
25	П	п	*p*	37	Ш	ш	*sh*
26	Р	р	*r*	38	Щ	щ	*shch*
27	С	с	*s*	39	Ъ	ъ’	*w*
28	Т	т	*t*	40	Ы	ы	*y*
29	Ћ	ћ	*chj*	41	Ь	ь	'
30	У	у	*u*	42	Ѣ	ѣ	*je*
31	Ў	ў	*uh*	43	Э	э	*eh*
32	Ф	ф	*f*	44	Ю	ю	*ju*
33	Х	х	*kh*	45	Я	я	*ja*
34	Ц	ц	*th*	46	Ѳ	ѳ	*f*
35	Ч	ч	*ch*	47	Ѵ	ѵ	*i*
36	Џ	џ	*dzh*	48		ж	*yh*

in Cyrillic characters when such words are used for zoological names (Note : It is not proposed
transliteration system should be annexed to the *Règles* for the guidance of zoologists when

in another English work it figures as " Yeisk ". As a result, even such an authoritative bibliographical guide as the *Zoological Record* uses different characters for the same Cyrillic letter. One of the favourite objects of the translators' " freedom of imagination " is the transcription of the most frequent termination for Russian and Bulgarian names, which is variously cited as " -ov ", " -ow ", " -off " or even " -of ".

3. We could give many examples of confusion created by such discrepancies as far as zoological and botanical names are concerned, but we shall quote here only one, which has been already generally discussed in the specialist press. Actually, one such case was taken as a subject by Dr. Helen Muir-Wood (1951 : 91) for her interesting article where she put forward the suggestion that the International Congress of Zoology should take a decision concerning the transliteration of words from languages using Cyrillic Alphabets, and that such a provision ought not to take the form of a recommendation but should be a binding directive.

4. Another zoologist, Dr. Paclt, who has published several papers on the problem of transliteration, expressed himself even more drastically (1950 : 998) by proposing to incorporate the transliteration system in the form of Appendix " H " in Article 19 of the *Règles Internationales de la Nomenclature Zoologique.* It is self-understood that this provision would then have to be adhered to as strictly as all the other Articles of the *Règles.*

5. Finally, the International Congress of Zoology has decided to include in the forthcoming revised text of the *Règles* a Schedule setting out the manner to be observed in transliterating words from the Cyrillic alphabets into the Latin alphabet when used as, or as part of, zoological names. We would, however, go even further by suggesting the necessity of including the standard transliteration system not only in the Rules of Zoological Nomenclature, but also in the Botanical Code and in Editorial Rules. It is desirable to unite the greatest possible number of publishers and periodicals in the use of a standard transliteration system, as only then can the chaos at present reigning in the transcription field be remedied. We do not think it necessary to enlarge on this subject, as sufficient matter has been already written on it, and we assume that no doubts are left on this account.

6. Of greater importance is the question of what form this standard system should take. All systems employed up to the present can be divided into two groups, in accordance with the way in which they transliterate those characters of the Cyrillic alphabets which have no counterpart in the Latin alphabet. The first of these systems is based on the principle of using of Latin letters surmounted by diacritic signs. The second is based on the principle of combining several letters of the Latin alphabet.

7. Viewed from a purely philological point of view, the two systems are equal in merit and in practice, most of the modern languagesusing the Latin alphabet employ both systems for the rendering of sounds which do not exist in Classical Latin. When dealing with the transliteration of languages using the Cyrillic alphabets, we cannot help but realise that the system of diacritic signs presents considerable advantage in that it is not bound to the phonetics of any particular non-Slav-language. Therefore it is usually employed in works on Slav literature and Slav philology written in non-Slav languages. Is it then to be wondered at if the Czechoslovak zoologist Dr. Paclt became an ardent adherent of this system, in view especially of the fact that the diacritic signs—the " hačeks "—are nearest to his psychology, being constantly used in his native tongue ? The transliteration system adopted by the Vatican library is based on the same principle.

8. Nonetheless, diacritic signs are seldom used in scientific works, and Dr. Paclt's assertion that " manche diackritische Zeichen (z.B. č, š, ž) . . . zur Romanisierung der kyrillischen Namen bereits offiziell benutzt werden " (1952 : 359) does not correspond with actual facts. As proof of this we could cite a long list of the most important bibliographical reference guides of various countries. In all of them the Cyrillic words have been transliterated without the use of diacritic signs. The following are a few examples of such publications : *Zoological Record* (Great Britain), *Bibliography and Index of Geology Exclusive of North America* (U.S.A.) ; *Bibliographie des Sciences Géologiques* (France) ; *Zentralblatt für Paläontologie* (Germany) ; *Boletín del Centro de Documentación Científica y Técnica* (Mexico) ; *Scientiae Naturalis Bibliographia* (Holland) ; *Boletín Bibliográfico Argentino* (Argentina). Russian authors also, if transcribing Cyrillic words into Latin characters, prefer with rare exceptions to avoid diacritic signs. This applies to the *Doklady* and *Izvestija* published by the Academy of Science as well as to other publications which appeared before 1947, in which year Soviet publications ceased to insert summaries and titles in foreign languages.

9. Moreover, Dr. Paclt's proposals appear to us dangerous insofar as by defending the general use of diacritic signs they route the quest for a standard transliteration system into the wrong channels. In fact, we completely fail to understand how Dr. J. Paclt, who recommends the replacement of diacritic signs by supplementary letters in the Hungarian, German and Scandinavian languages, can consider the same process as unsuitable for the Slav languages. And yet there are weighty practical reasons in favour of substituting letter-combinations for diacritic signs also in the Slav tongues.

10. It is evident that most printers do not possess the type for diacritic signs, and writers who wish to use such signs would normally not be in a position to do so. Such authors would then be forced to invent their own transliteration schemes " ad hoc ". Is there a question of a universal system ?

Dr. Paclt's phrase " Darum ist es immer zu wünschen, dass möglichst viele Druckereien die verschiedenen diakritischen Zeichen auf eine oder andere Weise reproduzieren können " (Ibidem) sounds altogether too optimistic. Unfortunately, neither the most ardent wish of one zoologist nor even the verdict of a Zoological Congress carries weight with the owners of printing works.

11. In this connection the following fact is of significance. The library of the United States Congress have worked out their own transliteration system and although the " hačeks " does not figure in it, other diacritical signs are employed. Nevertheless, when a large bibliographical work by R. Smits, namely the *Serial Publications of the Soviet Union* 1939–1950 was published, these signs were omitted, and the book appeared without them. This example shows clearly that, in spite of all the theoretical arguments which can be brought forward in defence of the transcription of letters of the Cyrillic alphabets by means of diacritic signs, this system can only be employed for special publications which have the corresponding printing types at their disposal. In all other cases (especially taking into account the additional inconvenience of using this system on typewriters) this method is quite unusable and cannot be accepted as a method for the creation of a standard international system.

12. But the most powerful argument against the system of diacritic signs is the decision of the Fourteenth Congress of Zoology, Copenhagen, 1953, to ban the use of diacritic signs in zoological names. It seems obvious that, if even in the practice of zoological nomenclature this system has to be banned, there is still more reason to drop it in library practice and in editorial work in general.

13. As regards the system of combining several Latin letters, up to the present its sole disadvantage lay in the fact that these combinations were made liberally and sometimes, as for instance in German, were exceedingly cumbersome (the rendering of one Cyrillic letter—the letter No. 38 of our table—demanded the use of seven Latin letters : " schtsch "). However, this one fault caused by striving to render the phonetic relationship between the Slav languages and those using the Latin alphabet, is easy to remedy by taking a letter without its own phonetic meaning (for instance, the letter " h ") and using it in the place of a diacritic sign, in order to change the phonetic meaning of the preceding letter. This solution is all the more adequate as it is already used in English transliteration practice where the combinations " ch ", " sh ", " zh ", and " kh " are employed.

14. For this reason, the transcription systems elaborated for the English-speaking countries, are fairly close to what ought to be the international system. The size and the aim of the present paper do not allow an extensive analysis of such systems from the philological standpoint. In practice, even the best among them, as for example that created by the Library of the U.S. Congress, are unfit for the present purpose because, while striving to adapt Slav phonetics to English sounds, they are compelled to introduce additional signs or to use the same Latin letter for several different Cyrillic letters. On the other hand, owing to the constant effort to be exact phonetically, the same Cyrillic letter, when it corresponds to different sounds in different Slav tongues is transcribed by means of different Latin letters, according to the phonetic requirements of the language concerned. For this reason, instead of one transliteration table one would have to prepare five tables, the use of which would be impossible for persons who are not acquainted with those languages. We have also noticed the inadequacies of the many transliteration methods now in use in scientific practice as well as the illogical ways in which these methods are applied. A superficial glance to the Russian quotations in the *Geophysical Abstract* prove this very convincingly.

15. Thus, in order that a transliteration system may actually become universal, it must, in our opinion, satisfy the following demands :—

(1) The system must be orthographic and not phonetic, i.e. each letter (and not sound) of the languages using the Cyrillic Alphabets must have its corresponding letter or combination of letters in the Latin Alphabet. This consideration is particularly important as its adoption will enable librarians and printers who have no knowledge of Slav languages to transcribe accurately words written with Cyrillic characters and thus to build up accurate card indexes. It is well known that at present even for the most elementary tasks connected with the Cyrillic alphabets the co-operation of persons knowing Slav tongues is required. Furthermore, the acceptance of this requirement will provide the possibility of an " inverted transliteration ", i.e. it will make it possible to establish the exact form in the Slav languages of names written with Latin letters and to locate them in alphabetical reference books in the original language. At present this process is at times very difficult.

(2) The system must be a system applying equally to the five Slav languages using the Cyrillic alphabets. Dr. Paclt asserts that " il est impossible de faire usage " en bloc " d'un seul tableau de romanisation servant à tous les alphabets slaves cyrillics " (1950 : 996). This assertion might have been correct, had it been our aim to render phonetically the letters of these languages. But, as our plan is to render them graphically, the preparations of such a table for the use of all five languages is quite feasible.

(3) The system ought to be international. The pronunciation for the characters used ought also to be definitely settled. Although the phonetic side of the problem appears to be of secondary importance,

nevertheless it is necessary to take as the basis the phonetic similarities with some one existing language. We have decided to choose English for this purpose as being the most suitable.

(4) Diacritic signs ought to be avoided on practical grounds.

16. The system which we have elaborated and which is represented on the annexed table, seems to meet satisfactorily all the above requirements. The most important divergence from systems now in use is that a single Latin letter or groups of letters is proposed for each Cyrillic letter, even where that letter has a different meaning in different Slav languages. For instance, Letter No. 4 in the annexed table has a different meaning in the Ukrainian and Belorussian languages from that in the other Slav languages. Similarly, Letter No. 8 has a different meaning in the Ukrainian and Serbian languages from that in the other Slav languages. Letter No. 13 is different in the Ukrainian language from the same letter in the other Slav languages. Letter No. 38 is pronounced differently in Russian and Ukrainian from the way used in Bulgarian. Letter No. 19 presents wide variations in different languages and dialects.

17. Nevertheless, these phonetic differences need not worry us, as they represent no difficulty to a person having a knowledge of the respective languages and are a matter of complete indifference to a person who does not know the language concerned. In different languages using the Latin alphabet the same letter also frequently has a different phonetic meaning, and yet nobody suggests that the name " Churchill " should be spelt as " Tschortschill " in German, or the name " Schumann " be spelt " Choumane " in French. On the other hand, the method which we recommend possesses the definite advantage that it enables anybody to transliterate a word correctly without knowing to which Slav language the word in question belongs.

18. As we have explained, we are keeping generally to the English language phonetics. We do this mainly because in practice the English method of transliteration affords a considerable economy in the use of letters for the forming of the combinations (almost everything is reduced to one " h "). Besides, it must be taken into account that the English language is so widely spoken at present that the English manner of writing Slav words has become well known and customary even for those people who do not possess a knowledge of that language. For example, this method of transliteration is very widely adopted in the Spanish-speaking countries. Actually, our only deviation from English phonetic rules is that we give the letter " j " the phonetic meaning which it has in German and the Slav languages using the Latin alphabet; it corresponds in phonetic value to the English letter " y ". In this way we succeed in avoiding the use of diacritic signs ; a result which the system of the U.S. Library of Congress failed to achieve owing to the effort which it made to adhere strictly to English phonetics.

19. The foregoing decision may seem inconsequent at first glance, as the letter " j ", corresponding to real Letters No. 16 and No. 17 in the table, has on the other hand the function of a " supplementary sign " when it is combined with " a ", " e " and " u ". Such criticism would be justified if such double usage would be a handicap for " inverted transliteration ". Hovever, in all five Slav languages corresponding sounds are represented in an absolutely definite way, thus eliminating the possibility of confusion. Analogous considerations are valid for the letter " w " which represents the Cyrillic letter No. 39 and enters in combination with " e " for rendering Letter No. 10. In this case also confusion is impossible because of the fact that the Letter No. 39 can never be preceeded by a vowel.

20. But such a confusion would be unavoidable if we were to represent Letter No. 34 by " ts " according to methods now in use, for Letter No. 28 is quite frequently followed by Letter No. 27 in Slav languages. We have therefore preferred to take " th " thus keeping " h " to its function of a supplementary sign.

21. Letter No. 41 does not correspond to any sound, it merely draws attention to a slight change in the pronunciation of the preceding sound. Therefore, while transliterating, this letter generally will not be designated at all in every-day routine, and among the scientific systems it is indicated at most by an apostrophe. It could readily be rendered by any Latin letter, for instance by " q ", but in this case already known geographical names, such as Kuban, Kharkov, would assume a rather puzzling and unusual look ; Kubanq, Kharqkov. In order to avoid a result of this kind, we decided to keep to the generally accepted method and to recommend the use of the apostrophe.

22. Finally, we would like to stress that, while elaborating our scheme, we were striving to keep it as simple as possible, transliterating the more frequent Cyrillic letters with the smallest practicable number of Latin characters.

23. The table which we submit comprises all letters existing in the Russian, Ukrainian, Belorussian,* Serbian and Bulgarian languages. Letters existing only in certain of these languages are placed in accordance with the order in which they are to be found in each of the alphabets of the langugages concerned. Thus, Letters Nos. 7, 17, 20, 23, 29 and 36 exist only in the Serbian language ; Letters Nos. 9 and 15, only in Ukrainian ; Letter No. 31, only in Belorussian ; Letter No. 48, only in Bulgarian ; Letters Nos. 46 and 47, only in the old Russian orthography ; Letters Nos. 10, 40 and 43 exist in Russian and Belorussian and the sign over Letter No. 10 is generally omitted in the Russian practice. Letter No. 5 is used only in Ukrainian and Belorussian ; Letter No. 39 only in Russian and Bulgarian ; Letter No. 42 only in the old Russian orthography and in Bulgarian ; Letter No. 38 only in Russian, Ukrainian and Bulgarian ; Letter No. 14 only in Ukrainian, Belorussian and the old Russian orthography. Letters Nos. 16, 41, 44 and 45 do not exist in Serbian.

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* This language is usually designated as " White-Russian ". We prefer the term " Belorussian " in order to avoid undue confusion in identifying an ethnical section of the Russian people with a purely political group.

24. In scientific routine it is usual to employ the Croat Latin alphabet for the transliteration of Serbian words, but we think it more suitable to include Serbian in the general system in view of the following considerations : (1) The Croat alphabet appears to us unsuitable, as it is based on the use of diacritic signs. (2) In journalism and every-day routine Serbian, words are often transcribed in accordance with phonetic similarities with the German, Italian and even English or Spanish languages. Thus, the situation of the Serbian language in practice differs little from other languages using Cyrillic alphabets.

25. In the case of all languages, except Russian, only the modern orthography is taken into account. The exception made in the case of Russian may be explained by the fact that the abolition of certain letters has taken place only a relatively short time ago (1919), and outside the Soviet Union, books are still being published using all or some of the abolished letters.

26. The following supplementary notes are added in regard to certain of the letters shown in the annexed table :—

Letter No. 10. In transliterations from the Russian language it is recommended that the letter " W " be omitted, as the sign " ·· " is omitted in the majority of original works.

Letters Nos. 14 and 42. These letters were abolished in the Russian orthography by the 1919 reform and should consequently be replaced by " i " and " e " respectively, always providing that the person who does the transliteration is sure that the word in question is Russian and not Bulgarian, Ukrainian or Belorussian.

Letter No. 17. This letter represents the way in which Serbs indicate the sound, which is indicated in the other Slav languages by the Letter No. 16.

Letter No. 39. This letter should be reproduced only in the middle of a word. It should be omitted if it figures at the end of the word in the original version. In some of the Soviet works (usually prior to 1929) this letter was replaced by the sign “ ’ ” and it should therefore be rendered also by “ W ”.

Letters Nos. 46 and 47. Both these are letters of the former Russian orthography, which are very rarely used. Thus, there is no need whatever to create special signs for rendering them, since even in Russian texts following the old orthography they are often replaced by Letters Nos. 32 and 13 respectively.

27. We give below a list of names of the Cyrillic letters arranged according to our scheme. Russian alphabet names are treated as being basic, the names in the other Slav languages are quoted only in the three following cases : (1) when a letter does not exist in Russian ; (2) when the phonetic value of a given letter differs from the Russian ; (3) when the name of a letter used in another language is substantially different from the name in Russian. Less important variations (as for instance “ fe ” for “ ef ” or “ khe ” for “ kha ”) are left out of consideration. The abbreviations used are the following “ U ” = Ukrainian ; “ BR ” = Belorussian ; “ B ” = Bulgarian ; “ S ” = Serbian ; “ ORO ” = old Russian orthography.

(1) a ; (2) be ; (3) ve ; (4) ge, U, BR—he ; (5) U, BR—ge ; (6) de ; (7) S—dje ; (8) je, U, S—e ; (9) U—je ; (10) jo ; (11) zhe ; (12) ze ; (13) i, U—y, ORO—double i ; (14) U, BR—i, ORO—single i ; (15) U—ji ; (16) brief i, U—ij ; (17) S—j ; (18) ka ; (19) el ; (20) S—lj ; (21) em ; (22) en ; (23) S—nj ; (24) o ; (25) pe ; (26) er ; (27) es ; (28) te ; (29) S—chje ; (30) u ; (31) BR—brief u ; (32) ef ; (33) kha ; (34) the ; (35) che ; (36) S—dzhe ; (37) sha ; (38) shcha, B—sht ; (39) hard sign, B—big jer ; (40) y ; (41) soft sign, B—small jer ; (42) ORO—jat’, B—double je ; (43) e ; (44) ju ; (45) ja ; (46) ORO—fita ; (47) ORO—izhitha ; (48) B—yh. Note : The letter “ j ” has the phonetic value of the English “ y ”.

28. **Appeal to interested specialists :** The creation of a logical and practicable transliteration system is of common interest to all parts of the international scientific world. It is very important therefore that any such system should enjoy the widest possible measure of support. It is accordingly very desirable that comments on our proposals should be as numerous as possible. It is important also that such comments should be furnished as promptly as possible, for a decision on the present matter must be taken by the International Commission on Zoological Nomenclature before the revised text of the *Règles Internationales de la Nomenclature Zoologique* can be promulgated, since under

the decision taken by the Fourteenth International Congress of Zoology, Copenhagen, 1953, the rules relating to the transliteration of words from the Cyrillic alphabets are to be incorporated in one of the Schedules to be annexed to the revised text of the *Règles*. The Secretary to the International Commission has invited us to act as a centre for the reception and collation of comments on, and suggestions regarding, the transliteration scheme submitted in the present paper, and for this purpose, on his recommendation, the International Trust for Zoological Nomenclature has made available to us a large supply of copies of the present paper for distribution. It is our particular hope, therefore, that as many interested specialists as possible will furnish us with statements of their views on our proposals. Statements so furnished may be written in any of the following languages : English ; German ; French ; Italian ; Spanish ; Portuguese ; any Slav language. All such communications should be addressed to Alexey Almasov (the senior author) at the following address : Avda. de Mayo 665, Piso 6, Buenos Aires, Argentina.

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DOCUMENT 26/3

Comment on the Almasov/Boltovskoy Plan relating to the transliteration of Cyrillic characters into the Latin alphabet

By **ERICH M. HERING**

(*Zoologisches Museum der Humboldt-Universität zu Berlin*)

(Statement received on 14th February 1955)

Es ist sehr begrüssenswert, dass die in die künftigen revidierten Regeln für die Zoologische Nomenklatur aufzunehmenden Vorschläge der Transliteration der cyrillischen Schriftzeichen so ausführlich zur Diskussion gestellt worden sind. Es wäre zu wünschen, dass sich auch alle Herausgeber wissenschaftlicher Zeitschriften einer solchen Regelung, sobald sie einmal vorliegen wird, anschliessen, damit eine weitgehende Einheitlichkeit in der Umschreibung solcher Namen durchgeführt wird. Es erscheint mir zweckmässig, das dabei noch einige Punkte hervorgehoben werden.

1. Die vorgeschlagene Transliterations-Empfehlung soll in keinem Falle rückwirkend angewendet werden, um die Stabilität der Nomenklatur nicht zu stören. Wo früher anstelle der nun einzuführenden Transliterations-Methode " diacritic signs " verwendet wurden, sollten diese künftig nur weggelassen werden, ohne dass Buchstabenveränderungen eintreten.

2. Die Transliterations-Vorschriften sehen die Wiedergabe cyrillischer Schriftzeichen in lateinischen Buchstaben oder Buchstaben-Kombinationen vor, mit deren phonetischem Wert in der englischen Sprache. Da nicht alle Sprachen in solcher Wiedergabe berücksichtigt werden können, ist die englische Sprache wegen ihrer Weltverbreitung dazu besonders geeignet, wenn diese Wiedergabe auch in der französischen und einigen germanischen Sprachen zunächst fremdartig wirkt. Da Transliteration in dieser Form in der Vergangenheit vielfach benützt worden ist, werden sich auch Angehörige von Nationen mit anderen Sprachen bald an sie gewöhnen.

3. Wichtig erscheint es mir aber, für die Umschreibungs-Vorschläge ein Prinzip zu betonen : Es ist in fast allen Fällen unmöglich, eine vollständig laut-getreue Wiedergabe der betreffenden Charactere zu erreichen, da diese mit allen Feinheiten doch nicht zu erzielen ist, daher auch entbehrlich ist. Es braucht also bei der Transliteration nur ein Annäherungswert in phonetischer Hinsicht erhalten zu werden. Deshalb können meiner Meinung nach gewisse Buchstaben-Kombinationen in dem Entwurf bei der künftigen endgültigen

Regelung weggelassen werden. Ich glaube, dass es nichts schadet, wenn verschiedene cyrillische Schrift-zeichen auch durch eine gleiche Kombination von lateinischen Buchstaben wiedergegeben werden, wodurch eine wesentliche Vereinfachung für den Benutzer erzielt wird. Es genügt wohl doch, wenn die vorgeschlagene Umschreibung dem cyrillischen Lautwert nahe kommt ; ihn vollständig zu erreichen, ist ja doch nicht möglich.

4. Unter Bezugnahme auf diese Ausführungen mache ich die folgenden Abänderuns-Vorschläge :

In der Transliterations-Tabelle mögen die durch die Ziffern bezeichneten cyrillischen Schriftzeichen wie folgt abgeändert werden :

No. 8. Dieses Schriftzeichen möge durch " je " wiedergegeben werden, da (im Gegensatz zu No. 43) bei der Aussprache deutlich ein j-Laut dem e vorangeht, so namentlich bei Stellung des Schriftzeichens am Beginn eines Wortes.

No. 10. Dieses Schriftzeichen muss unbedingt durch " jo " wiedergegeben werden, da das ë im Slavischen niemals als e gesprochen wird ; zudem wirkt die Wiedergabe des ë mit " ew " irreführend besonders dann, wenn auf das ë noch das cyrillische Schriftzeichen folgt, das in der deutschen Sprache als w (im Entwurf mit v wiedergegeben) umgeschrieben wird.

No. 13 und 14 klingen so ähnlich, dass in der Transliteration die Wiedergabe durch das eine zeichen i für beide Schriftzeichen ausreichend erscheint.

No. 39. Für das " Härte-Zeichen " schlage ich die vollständige Weglassung vor, da es ja auch in den slavischen Sprachen nicht ausgesprochen wird, sondern nur den vorhergehenden Konsonanten in einer Weise beeinflusst, dass er mit dem Klang erscheint, den er ohnehin in der englischen Sprache hat.

No. 41. Für das " Weichheits-Zeichen " schlage ich die Umschreibung mit " j " vor, das hinter den betreffenden Konsonanten zu stellen wäre. Das entspricht am besten dem phonetischen Wert dieses Zeichens. Die Verwendung eines' halte ich für ungeeignet ; dieses Zeichen kann dann leicht mit einem Apostroph verwechselt werden, so namentlich bei der Transliteration von Personen-Namen.

No. 43. Dieses Zeichen ist zweckmässig mit einem einfachen e zu umschreiben, da bei seiner Aussprache (im Gegensatz zu Zeichen No. 8) niemals ein j-Anlaut zu hören ist.

Zusammenfassend möchte ich empfehlen, dass man sich bei den Transliterationsvorschriften nicht scheuen soll, für sehr ähnlich klingende cyrillische Schriftzeichen einen gleichen lateinischen Buchstaben oder eine solche Buchstaben-Kombination zu verwenden, um eine leichtere Handhabung der Empfehlungen zu gewährleisten und leichteres Verständnis der Umschreibung zu erreichen. Es erscheint mir nicht wünschenswert, in die Buchstaben-Kombinationen willkürlich Zeichen wie h oder w einzufügen um anzudeuten, dass geringere phonetische Verschiedenheiten bestehen. Die Erreichung eines phonetischen Annäherungs-Wertes erscheint mir ausreichend.

DOCUMENT 26/4

Comment on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

By G. **WITENBERG**

(*Department of Parasitology, The Hebrew University, Jerusalem, Israel*)

(Letter dated 15th March 1955)

In response to your appeal[1] concerning the transliteration of Cyrillic transcriptions into the Latin alphabet, published in Part 1, vol. 11 of the *Bulletin of Zoological Nomenclature* I wish to express some views on this question, and I would be grateful if you could bring them to the notice of Dr. Almasov.

By proposing his thoroughly elaborated but rather revolutionary system of transliteration, it seems to me that Dr. Almasov did not follow the recommendation of the International Congress of Zoology to set up a method of transliterating words (that means sounds) written originally in the Cyrillic alphabet. Instead, his system implies the transliteration of the Cyrillic alphabet, not always exactly considering the sounds it represents. It seems to me that the transliteration should primarily be concerned with phonetics, while the original transcription should be of secondary value.

As pointed out by Dr. Almasov, pronunciation of some Cyrillic letters is different in various Slavic languages. It is evident, therefore, that their transliteration according to a rigid key common to all these languages might bring about distortion of their true characteristics. It seems, thus, that no such common key would be practicable. What we badly need is a system of transliteration of Cyrillic transcriptions for every Slavic language. I am not conversant with all of the Slavic languages, but I may judge the methods of transliteration of Russian transcriptions, and here I see a few difficulties in the system proposed by Dr. Almasov.

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[1] See paragraph 6 in Document 26/1.

(1) The transliteration of the Russian Ë by "ew" would be misspelled by all who do not know that this letter is pronounced in English like "yaw". Considering the Russian "approach", the leters "yo" or "jo" would be more appropriate.

(2) Transliteration of the Russian Ц by "th" instead of "ts" as is customary, would confuse all English speaking readers for whom "th" has a quite different sound; "ts" or "tz" seem to be more correct.

(3) Similarly, "tsh" seems to be more suitable than the proposed "ch" for transliteration of the Russian Ч.

I presume that similar confusing interpretations of the original pronunciation may be met in the transliteration of other Slav transcriptions, should a common key be accepted. I would, therefore, propose that a system of transliteration on the basis of pronunciation in every particular Slavic language but not transcription be worked out. This would possibly require efforts of experts in several Slavic languages, but such efforts would be worthwhile.

DOCUMENT 26/5

Comment on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

By **CURTIS W. SABROSKY**

(*U.S. Department of Agriculture, Agricultural Research Service, Entomology Research Branch, Washington, D.C., U.S.A.*)

(Letter dated 21st March 1955)

The article by Almasov and Boltovskoy on transliteration from languages using Cyrillic characters was indeed interesting. I cannot comment on it from a technical standpoint, but I certainly approve and applaud the idea of a system which would avoid the use of diacritic marks.

DOCUMENT 26/6

Comments by Professor TADEUSZ JACZEWSKI (Warsaw) on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

(a) Letter dated 29th March 1955

In connection with your " Notice to Zoologists and Palaeontologists " concerning the future schedule relating to the transliteration of words from the Cyrillic alphabets into the Latin alphabet (*Bull. zool. Nomencl.* **11** : 4–7, and with the communication of Drs. A. Almasov and E. Boltovskoy (*ibid.* : 7–18), I would like to express the following opinion.

I think the matter is very important and serious not only because the whole question requires urgently uniform regulation, but also as it is connected with effective international co-operation in the field of zoological nomenclature. I think, therefore, that it is absolutely impossible to take any decisions concerning these matters without a previous consultation with competent scientific institutions in those countries which use Cyrillic alphabets, i.e. the U.S.S.R., Bulgaria and Yugoslavia[1]. I think the Secretariat of the Commission should address corresponding letters to the Academy of Sciences of the U.S.S.R., to the Bulgarian Academy of Sciences, to the Yugoslavian Academy of Sciences and to the Serbian Academy of Sciences, asking their opinion as well as the opinion of their zoological institutions.

The proposals of Drs. A. Almasov and E. Boltovskoy are both very interesting and valuable, but I do not think it would be wise and even fair to decide these questions on the base of individual opinions only. It should not be forgotten that Cyrillic alphabets are used by over 200 million people and by thousands of zoologists. Moreover, it is not only the question of the five Slavonic languages, i.e. Russian, Ukrainian, Belorussian, Bulgarian and Serbian, but also of many other languages on the vast territories of the U.S.S.R., which were able to develop their literature since the Revolution of 1917 and which use alphabets based to a greater or smaller extent on the Cyrillic alphabet.

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[1] For the action taken by the Office of the Commission with a view to eliciting the views of the leading institutions concerned in the Slav-speaking countries see paragraph 6 of Document RR/1 and the list of Institutions given in Part (b) of the Appendix to the paper so numbered.

(b) Letter dated 29th April 1955

I was very glad to learn that you quite agree with me as to the consultation with competent scientific institutions in the U.S.S.R. and other interested countries in questions concerning transliteration from the Cyrillic into the Latin alphabet.

Personally I have also some technical remarks in connection with the schedule proposed by Drs. A. Almasov and E. Boltovskoy (*Bull. zool. Nomencl.* **11** : 7–18), which I would like to communicate to you and to put under discussion. They are as follows :—

(1) Letter 8 should be transliterated in Russian words *je* and not *e* ; it has the phonetic value of *e* only in Ukrainian, but not in Russian and in Bjelorussian.

(2) Letter 10 should be transliterated *jo* ; in both these cases, i.e. letters 8 and 10, *j* has the phonetic value which it has in German or in the Slavonic languages using the Latin alphabet (as Polish, Czech, etc.).

(3) Letter 34 should be transliterated simply by *c*, giving to it the phonetic value which it it has in such German words (names) as Cäcilie, Cäsar, or in the Slavonic languages using the Latin alphabet, i.e. the phonetic value of *ts*.

(4) The letter 39 can be simply omitted in all Russian words ; it is used only to indicate that the preceding consonant is not palatized, and there will be no such danger when the word will be transliterated into the Latin alphabet.

DOCUMENT 26/7

Comment by Dr. JOSHUA L. BAILY, Jr. (San Diego, California, U.S.A.) on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

(Letter dated 7th May 1955 addressed to Dr. Alexey Almasov)

Your very thorough and carefully thought out article in the *Bulletin of Zoological Nomenclature* (**11** : 7 *et seq.*) concerning the transliteration of words from languages using the Cyrillic alphabets is very thought-provoking. The need for a standard method of transliteration is quite obvious, not only for words taken from the Slavic languages, but for the languages of Western Europe which use the Latin alphabet supplemented by additional letters which do not occur in Latin.

I find myself in rather fundamental disagreement with you, however, when you recommend that the system should be orthographic rather than phonetic. Most of the spoken languages of Europe (all of those with which I am familiar, except English) are phonetic, which is a great help to anyone who has to use them. The one exception is English, in which etymological spelling is used. Since so many words in common use in English have come originally from other languages, the use of orthographic spelling has brought about the representation of many different sounds by the same letter or combination of letters, and also of the representation of the same sound by different letters or combination of letters, with a most unsatisfactory resulting confusion. It would be most disastrous if such a condition were allowed to develop in modern scientific Latin, but this is almost sure to happen, if the system of transliteration adopted by the International Commission should be orthographic rather than phonetic.

The only advantage that I can see in using an orthographic system is that the letters of all five Cyrillic alphabets can be entered in one column, whereas in a phonetic system each alphabet would need a separate column. But this would offer no insuperable difficulty. As a matter of fact, the most important of all the Cyrillic alphabets was not used in compiling the tables in your article. I refer of course to the Greek alphabet. It might be maintained that since the

Greek alphabet had been in use for many centuries before the birth of Cyril, that it should not be called a Cyrillic alphabet, but that is an academic argument. When Cyril invented the alphabet that bears his name he used the characters of the Greek alphabet as far as he could to designate the usual sounds associated with them, and going to the old Phenican alphabet to get signs, to represent sounds that did not occur in Greek, so that the Greek and Cyrillic alphabets consist largely of the same letters.

In Greek the letter θ is always transliterated by TH so that the sound may be preserved. The same letter occurs in the Cyrillic alphabets, and if the system of transliteration is to be orthographic the same digraph should be used. But your scheme provides that θ be represented by F. This is not an orthographic but a phonetic equivalent. I agree with you completely that θ in Slavic languages should be transliterated by F in order to preserve the sound, but I think it should be transliterated by TH in Greek words for the same reason. It would be quite impractical to change TH to F in all words of Greek origin in scientific Latin. The use of TH must be considered a fixture.

Consequently I must also disagree with your recommendation to use TH as the orthographic equivalent of Ц the sound of which is altogether different. The latter letter should be rendered as TS in order to preserve the sound. Your objection to the use of TS on the ground that many Slavic words contain this digraph which would be transliterated in the same way seems immaterial to me, but if it is a legitimate objection it might be satisfied by the use of TZ for Ц.

Again, the Greek letter X has always been transliterated by CH in Latin. I agree with you that the digraph KH is more logical, but to change the spelling of every Latin word of Greek origin to comply with this would be quite impractical. I therefore, recommend the use of CH in Slavic words as well as in Greek words, since the sound is the same in both cases. If the sound were different I would recommend KH for Slavic words.

If this suggested change is accepted it will be necessary to abandon the digraph CH as the equivalent of Ч. It would better be represented by the trigraph TSH. I may say that I do not like the digraph CH because it is ambiguous. In English C may be either a mute or a sibilant. The following vowel always determines which, so there is no confusion. H following C is used both to fricatize the mute and to dentalize the sibilant. Any vowel may follow the digraph in either case, so that it is impossible to tell in which sense it is used. That is why I prefer TSH for the dentalized sibilant.

The letter B in the Cyrillic alphabets is orthographically the same letter in Greek, which is transliterated as B. But it has not the same sound. I agree with you that in the transliteration of Slavic words it would be better to represent it by V but this is its phonetic, not its orthographic, equivalent.

The difference in sound of the myakhy-znak and the tvyordy-znak is so slight that I would recommend dropping these altogether in transliteration.

There are some other details which I think capable of improvement, but all of them have come about by the attempt to make the transliteration orthographic instead of phonetic. After all, a word is a sound pronounced by a speaker ; it is not a collection of symbols on a printed page. The object of the printed symbols is to represent the spoken sounds. When they cease to do this, their value disappears.

The advantage of writing scientific terms in Latin is that this is supposed to be something of a universal language, understood by scientists all over the world. The practice of scientists of pronouncing Latin words in accordance with the orthography of their own language is not to be recommended, as it frequently makes it impossible for scientists who speak different languages to understand each other. If a phonetic system of transliteration were adopted, the original pronunciation would be preserved, and would be universally understood.

Of course, the important thing to achieve is a system of transliteration, the rules of which will be simple enough to be remembered so that any one can understand it, whether it is orthographic or phonetic. For my own part, I would much prefer a phonetic system, but of course I shall abide by the decision of the Commission. I hope you do not mind my having expressed myself so vigorously.

DOCUMENT 26/8

Views of the Academy of Sciences of the U.S.S.R. on the Almasov/Boltovskoy Plan for the transliteration of the Cyrillic Characters

(Letter dated 5th September 1955 from D. M. Steinberg,
Vice-Director of the Zoological Institute of the Academy of
Sciences of the U.S.S.R.)

As it is rather difficult for zoologists to discuss the question of transliteration of words written in Cyrillic characters we sent over your letter and the paper of M. M. Almasov and Boltovskoy to the Institute of Linguistics of the Academy of Sciences of the U.S.S.R. so as to have the opinion of this special Institution on behalf of the project.

We have the pleasure to enclose herewith a copy of the answer we received a few days ago, which we hope may be of some use to you.

As to what concerns the opinion of the Zoological Institute of the Academy of Sciences of the U.S.S.R., we find the scheme proposed by M. M. Almasov and Boltovskoy, in the part concerning the Russian alphabet, quite acceptable with the exception of the letter No. 39 (Ъ), which we think more desirable to succeed by the sign (“), when used in the middle of words and omit, when used at the end of words.

At the proposals of M. M. Almasov and Boltovskoy concern letters used not only in the Russian alphabet, but also in the Ukrainian and Belorussian languages we would think it very desirable if you would find the possibility to send a request to the Ukrainian Academy of Sciences (Kiev, the Ukrainian S.S.R.) and the Belorussian Academy of Sciences (Minsk, the Belorussian S.S.R.).

ANNEXE TO DOCUMENT 26/8

Opinion of the Bureau of the Section of General and Comparative Linguistics of the Institute of Linguistics of the Academy of Sciences of the USSR (Moscow) on the project of the System of transliteration of words normally written in Cyrillic characters proposed by the International Commission on Zoological Nomenclature.

1.- The project of transliteration proposed by MM. Almasov and Boltovskoy cannot be recommende in view of the following:

1) it runs contrary to the international project worked out by the ISO (the latest version we know of is of May 1954);

2) in its character it is not international but regional, as it is chiefly based on the Anglo-American system of RGS;

2.- The divergencies with the ISO system come up to the following :

1) the use of digraphs for hissing consonants: No 11 zh - ж , No 35 ch - ч ,No 37 sh - ш and No 34 th - ц as well;

2) the use of trigraphs : No 29 chj - ћ ,No 36 dzh - џ ;

3) the use of tetragraphs : No 38 shch - щ ;

4) the symbols : No 5 gh - ґ , No 9 jeh - є , No 10 ew - ё , No 14 ih - і , No 31 uh - ў , No 39 w - ъ (evidently not only for Russian, but for Bulgarian texts as well) , No 42 je - ѣ No 43 eh - э .

Altogether 15 cases out of 48.

3.- These discrepancies reveal complete disregard for the traditions of Latin transliteration of Slavonic texts (for ex.: No 34 th - ц , No 10 ew - ё , No 14 ih - і , No 31 uh - ў , No 43 eh - э , No 39 w - ъ) .

4.- The fact that the linguistic and graphic treatment of the matter is not thorough in the proposed system is revealed, for instance, in the following cases :

1) No 39 w - ъ ; in the Russian language, the "separating symbol" ъ " in the middle of words marks the "j-like" beginning of the following vowel (thus объем - objom , etc..) It would be difficult to conceive transcriptions with "w" instead of " ъ" and " ew " for ъ ё ъ (obwewm). On the other hand, in Bulgarian texts " ъ " stands for " ѫ ", which is a separate vowel. According to the ISO transliterations scheme, it should be rendered as " ǎ " .

2) The symmetrical and parallel symbols for occlusive palatal consonants in the Serbian language, ђ - voiced and ћ - voiceless, become unsymmetrical : No 7 ђ - dj , No 29 ћ - chj.

3) The proposal of using "h" and "w" as diacritic signs in digraphs and trigraphs is quite inconsistant : No 31 uh - ў (why not "uw", which might be used provided digraphs were generally accepted

No 34 th - ч , No 43 eh - э , No 14 ih - і , No 48 yh - Ж and No 9 yeh - є on one hand, and No 10 ew - ë on the other.

5.- The Institute of Linguistics of the Academy of Sciences of the USSR, basing itself on the traditions of the Czech Latin alphabet ("latinitsa") widely employed for Latin transcription of sounds of the Slavonic languaged in the linguistic literature, as well as on the traditions of the "Academical transcription of Russian proper names with Latin letters" of 1906 (improved version of 1925 in accordance with the new Russian orthography), has proposed its own project of Latin transliteration of languages using the Cyrillic alphabet. This project in the main features coincides with the ISO project and differs from it only in the following points:

	I S O	The Institute of Linguistics of the Academy of Sciences of the USSR
г	Ukrainian and Byelorussian - g	h
х	h (and optionally ch, kh)	ch
е	always e	e -after consonants; je - at the beginning, after vowels, after ъ and ь .
ё	Ё	'o - after consonants; jo - at the beginning, after vowels, after ъ and ь .
ю	always ju	'u - after consonants; ju - at the beginning, after vewels, after ъ and ь .
я	always ja	'a - after consonants; ja - at the beginning, after ~~consonants~~ vowels, after ъ and ь .
и	always i	i - at the beginning; after consonants and vowels; ji - after ь .
ъ	Russian : "or"-in the middle omitted at the end	always omitted
ь	ˋ or ˊ	at the end and before consonents; omitted before vowels.
љ	Serbian lj	l'
њ	" nj	n'

After a perusal of the second version of the ISO project (May, 1954) and in connection with the above mentioned divergencies, the

Institute of Linguistics forwarded in September 1954 its suggestions to the ISO through the Committee for Standardization attached to the State planning Commission of the USSR.

6.- The Institute of Linguistics is of opinion that it would be very desirable if the International Commision would take acquaintance with the latest project of ISO (Geneva,39,Route de Malagnou) and would coordinate the conclusions,which should have internatio - nal significance,with the International Organization for Standardization (ISO).

Signed (A.A.Reformatsky)

DOCUMENT 26/9

Comment on Almasov/Boltovskoy Plan relating to the transliteration of Cyrillic Characters

By **LEO SHELJUZHKO**

(*Zoologische Sammlung des Bayerischen Staates, Entomologische Abteilung, München, Germany*)

(Letter dated 9th December 1955)

Besten Dank für Ihr freundliches Schreiben vom 17.XI., wie auch für die Zusendung des Sonderdruckes des Artikels von Dr. Almasov und Boltovskoy mit den Vorschlägen zur Transkription des cyrillischen Alphabet. Leider kann ich mich manchen Ansichten der genanten Autoren nicht anschliessen. Deshalb habe ich einen Artikel verfasst, in dem meine entsprechenden Ansichten dargelegt sind und habe diesen, Ihrem Vorschlag entsprechend, Dr. Almasov zugesandt. Eine Abschrift davon erlaube ich mir diesem Brief beizulegen.

Über die internationale Transkription russischer Worte

(Zum Artikel von A. Almasov und E. Boltovskoy : "On the treatment of words written with cyrillic characters, for the purposes of zoological nomenclature, bibliography, reference indices, etc." (*Bull. zool. Nomencl.*, Vol. 11, Part 1, 1955)).

1. Allgemeine Bemerkungen

1. Im vorliegenden Artikel wird die Transkription nur russischer Worte berücksichtigt. Die anderen slavischen Sprachen, die sich des cyrillischen Alphabet bedienen, sind mir nicht genügend bekannt, um über deren Transkription zu diskutieren ; auch wage ich nicht zu entscheiden, ob sie alle bei der Transkription auf einen Nenner gebracht werden können. Gewiss wäre dies an und für sich erwünscht, doch, meiner Meinung nach, nicht unbedingt erforderlich, jedenfalls nicht auf Kosten einer falschen Aussprache der transkribierten Worte.

Wenn sich Almasov und Boltovskoy für eine einheitliche Transkription der cyrillischen Buchstaben aller 5 slavischen Sprachen einsetzen, so sehen sie doch selbst ein, dass hier eine absolute Konsequenz nicht gut möglich ist und dass man schliesslich doch gewisse Eigentümlichkeiten der verschiedenen Sprachen berücksichtigen muss. Das betrifft (*l.c.*, p. 16, Nr. 26) die russischen Buchstaben : "ë", "i", "ѣ" und "ъ". Nun glaube ich, dass dieser Weg unvermeidlich ist. Meiner Meinung nach müsste man also für die fünf in Frage kommenden slavischen Sprachen entsprechende Transkriptionsschemen ausarbeiten und dann diese Schemata miteinander vergleichen und die Eigentümlichkeiten jeder Sprache berücksichtigen. Die Unterschiede werden nicht gross sein und nur einzelne Buchstaben betreffen. Es liese sich dann ein allgemeines Schema aufbauen, in dem die Abweichungen der einzelnen Sprachen notiert wären. Obwohl dies die Sache auch etwas komplizieren würde, wäre in solcher Weise ein wirklich gut gebräuchliches Schema geschaffen, anstatt eines einfacheren, da alle Eigentümlichkeiten der Sprachen nivelliert und daher unverständlich und kaum gebräuchlich wäre. Wenn die erwähnten Autoren die Vorteile eines allgemeinen Schemas für die Bibliotheken hervorheben, da solch ein Schema die Möglichkeit geben würde, die in cyrillischen Schrift geschriebene Worte zu transkribieren, ohne die entsprechenden Sprachen zu kennen (*l.c.*, p. 13, Nr. 15 (1)), wäre dagegen einzuwenden, dass man wohl annehmen muss, dass eine Bibliothek mindestens wissen sollte, in welcher Sprache eine Schrift verfasst ist (was ja eine minimale Forderung wäre!) und wenn dies bekannt ist, so hätte es auch keine Schwierigkeiten, in der Transkription dem entsprechenden Schema zu folgen.

2. Vollkommen einverstanden bin ich mit den erwähnten Autoren in der Ablehnung der diakritischen Zeichen. Erstens aus dem Grunde, da diese nicht ohne Weiteres verständlich wären, zweitens darum, da die meisten Druckereien über solche Zeichen nicht verfügen und also deren Anwendung, praktisch genommen, nur in den wenigsten Fällen möglich wäre, wodurch das ganze Transkriptionssystem zu Fall gebracht wäre.

3. In diesem Artikel geht es also darum, russiche Worte durch lateinische Buchstaben zu transkribieren, wobei als Grundlage dieser Transkription die Empfehlung aufgefast wird, die als "Anhang G" der Zoologischen Nomenklaturregeln (Richter, 1948, p. 210) angeführt ist: "Man drücke . . . möglichst genau die örtliche Aussprache der Namen aus, ohne jedoch eine vollständige Wiedergabe der gehörten Laute in Anspruch zu nehmen".

Bei einer Transkription müssen wohl vor allem die Interessen der Internationalen Nomenklatur berücksichtigt werden und hier geht es in erster Linie um die Wiedergabe von Eigennamen, sei es Personennamen oder geographische Bezeichnungen. Es ist verständlich, dass diese Namen auch in phonetischer Hinsicht der Originalaussprache möglichst genau entsprechen sollen und nicht durch eine primitive, alles nivellierende Schreibweise verunstaltet werden.

Es handelt sich also nicht nur darum, russische Buchstaben durch lateinische zu ersetzen, sondern auch darum, die Phonetik nach Möglichkeit zu berücksichtigen. Dies widerspricht den Ansichten von Dr. Almasov und Boltovskoy (*l.c.*, p. 13, Nr. 15 (1)), die behaupten, dass "The system must be orthographic and not phonetic". Diese Behauptung steht nicht im Einklang mit der Empfehlung der Nomenclaturregeln, die oben zitiert wurde.

Wenn die erwähnten Autoren die Phonetik auch ablehnen oder ihr jedenfalls eine zweitrangige Bedeutung zumessen, sagen sie doch (*l.c.*, p. 14, Nr. 15 (3) : " . . . nevertheless it is necessary to take at the basis the phonetic similarities with some of existing language. We have decided to choose English for this purpose the most suitable". Das wäre eine Ansicht, die keinesfalls einläuchtend ist. Es ist gewiss klar, dass man die Aussprache der cyrillischen — lateinisch transkribierten — Worte an die Aussprache einer bestimmten Sprache binden muss, ebenso klar ist es aber, dass diese Sprache nur die lateinische sein kann—eine neutrale Sprache, die in der Nomenklatur eine weitgehende Verwendung findet und die es auch jedem ermöglichen wird, die lateinisch transkribierten Worte ohne Kenntniss der slavischen Sprachen richtig auszusprechen. Es wäre aus manchen Gründen durchaus nicht angebracht, eine moderne Sprache hier zu benutzen, da jede von diesen ihre Eigentümlichkeiten hat, die nicht im Einklang mit der lateinischen stehen. Ganz besonders abwegig wäre die Anwendung der englischen Sprache, die besonders viele Eigentümlichkeiten in der Aussprache besitzt, die weder mit den slavischen Sprachen, noch mit der lateinischen Sprache etwas gemeinsames haben.

Es wäre zu bedenken, dass die Eigentümlichkeiten der englischen Aussprache nicht nur einige umstrittene Buchstaben, sondern auch viele andere betreffen. Sollte man die englische Transkription für einige Buchstaben annehmen, so wäre damit die Tendenz gefördert, auch die übrigen in englischer Leseart auszusprechen, womit die Originalaussprache gänzlich verunstaltet wäre. Manche Engländer und Amerikaner haben freilich auch jetzt die Gewohnheit, lateinische Namen auf englische Art auszusprechen, doch muss man zugeben, dass dies eine schlechte Gewohnheit ist und man sich diesem Fehler unmöglich anschliessen kann, geschweige den ihn zum Prinzip zu erheben !

Diese Entgegnung gilt selbstverständlich auch dem Vorschlag von Almasov und Boltovskoy (*l.c.*, p. 12, Nr. 13) : "This solution is all more adequate as it is already used in English transliteration practice where the combinations 'ch', 'sh', 'zh', and 'kh' are employed". Meine Einwände beschränken sich hier auf zwei Fälle, nämlich auf die Kombinationen "ch" und "kh". Das "ch" ist in der lateinischen Sprache gebräuchlich, kann also in transkirbierten Worten nur in einer Weise verwendet werden, die der lateinischen Aussprache entspricht, also als russisches "х" und keinesfalls als russisches "ч", wie das von den genannten Autoren vorgeschlagen wird. Wenn in englischen Worten, die selbstverständlich ihre Originalschreibweise beibehalten, dies "ch" als russisches "ч" ausgesprochen wird, und in der französischen als russisches "ш", so ist es etwas ganz anderes und macht auch keine Schwierigkeiten bei der Aussprache, vorausgesetzt, dass man weiss, dass es sich um ein englisches oder französisches Wort handelt.

Das "kh" ist, wie es auch in den Nomenklaturregeln angegeben wurde (Richter, 1948, p. 210) ein weicher arabischer Kehllaut, der etwa einem "gh" entspricht. Es würde sehr verwirrend wirken, sollte man plötzlich dieses "kh" dem russischen "x" gleich stellen.

Wegen den zwei weiteren Kombinationen hätte ich keine Einwände. Das "zh" ist weder in der lateinischen noch in der englischen Sprache gebräuchlich ; seine Anwendung für das russische "ж" ist künstlich und bedingt, womit man sich jedoch abfinden muss, zumal wir in der lateinischen Sprache keinen entsprechenden Äquivalent finden. (Dasselbe gilt auch für die Anwendung des "y" für das russische "ы"). Auch gegen die Anwendung von "sh" für das russische "ш" wäre nichts einzuwenden : das "sh" kann wohl kaum anders als "sch" ausgesprochen werden, ist also allgemein verständlich und hat sich auch seit langer Zeit in der Transkription fest eingebürgert.

Wenn sich die genannten Autoren für die englische Sprache so einsetzen, so wäre es nur logisch, die englische Transkription als internationale anzunehmen — ein Vorschlag, der aus vielen Gründen kaum eine allgemeine Anerkennung finden könnte und bei dessen Durchführung man schon wegen der weitgehenden Eigentümlichkeiten der englischen Aussprache auf unüberwidliche Schwierigkeiten stossen würde.

4. Es wäre wohl angebracht, bei der Aufstellung einer internationalen Transkription nach Möglichkeit das beizubehalten, was in dieser Hinsicht bereits getan wurde und dies jedenfalls nicht einfach zu ignorieren. Man muss bedenken, dass die Fragen der lateinischen Transkription russischer (und überhaupt cyrillischer) Worte nicht erst heute aufgetaucht sind. Es sind vielmehr Fragen, mit denen man sich schon Jahrzehnte befasst hat und die Ergebnisse dieser Arbeit müssen wenigstens genau untersucht und nicht ohne schwerwiegende Gründe einfach verworfen werden, schon deshalb nicht, da manche dieser Ergebnisse sich inzwischen in der Praxis fest eingebürgert haben und jede Änderung eine Umwälzung zur Folge hätte, die nicht gerade wünschenswert wäre, besonders dann nicht, wenn sie keiner Notwendigkeit entspricht.

Sollte man sich die Mühe geben, die russische wissenschaftliche Literatur des letzten Jahrhunderts durchzusehen, so könnte man die Evolution feststellen, die die Transkription russischer Worte erfahren hat. Es ist gewiss hier nicht der Platz auf die Einzelheiten einzugehen und diese Evolution zu verfolgen. Ich beschränke mich auf die Erwähnung der Arbeit von N. Kusnezov (Faune de la Russie, Insectes Lépidoptères, Vol. I, Livr. 2, p. CCCLXXXXVII. Leningrad. 1929), in der u.a. die lateinische Transkription von mehreren russischen Buchstaben angeführt ist, nämlich: "в = v, ж = zh, x = ch, ц = tz, ч = tsh, ш = sh, щ = stsh, ю = ju, я = ja". Es ist dabei zu betonen, dass diese Schreibweise nicht von Kusnezov *ad hoc* erfunden wurde, sondern nur eine Zusammenfassung dessen darstellt, wozu die russischen Autoren im Laufe von Jahrzehnten gelangt sind. Nach dem Erscheinen dieser Arbeit haben sich wohl die meisten russischen Autoren an diese Transkription gehalten und man kann wohl annehmen, dass sie jetzt eine allgemeine Anerkennung findet. Diese Transkription hat sich gut bewährt, widerspricht keinesfalls den Forderungen der Nomenklaturregeln und es besteht durchaus kein Grund sie umzustossen und durch eine andere, der englischen Sprachweise entsprechende zu ersetzen.

Es besteht übrigens die Gefahr, sollte man für die Transkription ein widernatürliches, vollkommen künstliches System vorschlagen, wie dies die englische Schreibweise wäre, dass manche Autoren sie nicht akzeptieren würden, und dass daher die erstrebte Einigkeit nicht erreic wäre.

Nach diesen allgemeinen Bemerkungen gehe ich jetzt zur Transkription einzelner Buchstaben über, insofern ich mit den entsprechenden Vorschlägen der Herrn Dr. Almasov und Boltovskoy nicht einverstanden bin.

II. Transkription der einzelnen Buchstaben

1. Das russische "e" entspricht nicht genau dem lateinischen (oder deutschen) "e". Es handelt sich vielmehr um einen jotierten Laut, der wohl am besten durch ein "je" wiederzugeben wäre. Jedoch kommt diese Jotierung nicht immer gleich deutlich zum Vorschein. Besonders fällt die Jotierung auf, wenn das Wort mit einem "e" beginnt, oder auch wenn es inmitten des Wortes, aber vor einem Vokal steht. In diesen Fällen würde es sich empfehlen, es durch "je" wiederzugeben, so z.B. : "Jegorov, Jermolajev, Jershov, Dostojevskij, Jelabuga, Jeletz, Kijev" (nicht : "Egorov, Ermolaev, Ershov, Dostoevskij, Elabuga, Eletz, Kiev"). Dagegen braucht die Jotierung inmitten des Wortes vor einem Konsonanten nicht hervorgehoben zu werden, kann also in diesen Fällen dem lateinischen "e" gleich gestellt werden, wie z.B. : "Tsherskij" (nicht "Tshjerskij").

2. Das verhältnismässig seltene russische "ë" entspricht recht gut dem lateinischen "jo" und wäre am besten so wiederzugeben, z.B. "Orjol". Da sich aber die Schreibweise dabei bedeutend verändert (es kommt ein "o" anstatt des "e"), erscheint es ratsam auch die zweite Schreibweise in Klammern beizufügen, also : "Orjol (Orel)". Der Vorschlag das "ë" dem "e" gleich zu stellen widerspricht der Aussprache. Da aber, wie bereits erwähnt, das "ë" in der russischen Sprache nur eine seltene Erscheinung ist, hat die Differenz keine grosse Bedeutung.

3. Dem Vorschlag das russische "c" durch "s" wiederzugeben, kann man gewiss nur zustimmen, jedoch mit einer Einschränkung. Es entstehen nämlich in den Fällen Bedenken, wo das russische "c" zwischen zwei Vokalen steht. In diesen Fällen besteht die Gefahr, dass das "s" als russisches "з" ausgesprochen wird. Um das zu vermeiden erscheint es ratsam, es hier zu verdoppeln, so müsste man schreiben : "Lomonossov", "Ossa" (anstatt : "Lomonosov", "Osa").

4. Wie es bereits im allgemeinem Teil erwähnt wurde, entspricht das russische "x" vollkommen dem lateinischen "ch" und kann naturgemäss nur

in dieser Weise wiedergeben werden. Den Vorschlag es durch “kh” wiederzugeben, möchte ich als völlig abwegig bezeichnen, da dies mit der Aussprache durchaus nicht übereinstimmt, wie auch mit den Empfehlungen der Nomenklaturregeln. Also : Cholm, Charkov, Cherson (nicht : “Kholm, Kharkov, Kherson”).

5. Das russische “ц” ist eigentlich eine Kombination zweier Buchstaben : “т” und “з”, dementsprechend müsste es so transkribiert werden, wie man diese Buchstaben transkribiert, also “tz”. Die vorgeschlagene Transkription “th” ist absolut unannehmbar. Sie kann weder mit der russischen noch mit der lateinischen Aussprache in Einklang gebracht werden und auch die englische Aussprache entspricht der russischen nicht genau. Wenn wir z.B. “Tzaritzin” oder “Tzarevokokshajsk” schreiben, so ist dies allgemein verständlich, dagegen aber wäre “Tharithin” oder “Tharevokokshajsk” überhaupt kaum lesbar und sogar für einen Russen unverständlich.

6. Das russische “ч” ist auch nichts weiter als eine Kombination von zwei Buchstaben, nämlich von “т” und “ш” uns soll dementsprechend als “tsh” wiedergegeben werden, was auch gebräuchlich ist. Der Vorschlag es durch “ch” zu schreiben, wäre absolut unannehmbar, wie ich es bereits erwähnt habe. Erstens, schon deshalb, da das lateinische “ch” dem russichen “х” entspricht ; zweitens, da eine englische Leseart durchaus unangebracht und verwirrend wirken würde. Also : “Tshetverikov, Tshitsherin, Tshita, Tsheljabinsk” (nicht : “Chetverikov, Chicherin, Chita, Cheljabinsk”).

7. Das russische “щ” ist wieder eine Kombination zweier Buchstaben, nämlich bon “ш” und “ч” und müsste dementsprechend als “shtsh” geschrieben werden. In der Praxis aber wird eine Abkürzung gebraucht und wird dieses “щ” als “stsh” transkribiert. Gegen diese Abänderung wäre nichts einzuwenden, zumal sie die Aussprache nicht betrifft : “shtsh” und “stsh” können nur nahezu identisch ausgesprochen werden. Der Vorschlag, das “щ” als “shch” wiederzugeben beruht auf der nicht annehmbaren Transkription von “ч” durch “ch” und ist daher zu verwerfen. Also, hätten wir : “Stshegolkov, Stshogolev, Stshelkanovtzev” (nicht : “Shchegolkov, Shchogolev, Shchelkanovtzev”).

8. Das russische harte Zeichen “ъ” kommt für die Transkription nur in den seltenen Fällen in Frage, wo es inmitten der Worte steht. (Am Ende der Worte ist es stumm und braucht nicht transkribiert zu werden. Auch wird es nach der neuen Grammatik hier nicht mehr gebraucht). Es ist gebräuchlich und erscheint auch zweckmässig das “ъ” inmitten der Worte durch einen “ ’ ” wiederzugeben. Der Vorschlag es durch “w” zu transkribieren (*l.c.*, p. 17, Nr. 26) ist unverständlich, wirkt verwirrend und erscheint völlig zwecklos. Was das bulgarische “ъ” betrifft, so scheint seine Leseart etwas anders zu sein und müsste man sich überlegen, ob man hier nicht eine andere Schreibweise anwendem müsste.

9. Das russische weiche Zeichen “ь” macht gewisse Schwierigkeiten und zwar darum, da seine Wiedergabe nicht in allen Fällen notwendig erscheint. Dort aber, wo seine Anwendung nötig ist, wäre es wohl am besten, es durch “j” zu transkribieren, was auch der Praxis entspricht. Dies soll wie am Ende der Worte, so auch inmitten derselben geschehen, so z.B. : “Kazanj, Permj, Obj, Gorjkij” (anstatt : “Kazan, Perm, Ob, Gorkij” wie es oft geschrieben wird). In manchen Fällen aber — nach einem “l” — scheint das weiche Zeichen überflüssig zu sein und könnte wegbleiben, so z.B. : “Olga, Olgopol, Olviopol, Jelisavetpol, Jaroslavl” (anstatt : Oljga, “Oljviopolj, Jelisavetpolj, Jaroslavlj”). Wenn aber das weiche Zeichen “ь” zwischen dem “l” und einem Vokalen steht, so wäre es unbedingt zu berücksichtigen, z.B. : “Iljinskij, Iljitshev” (nicht : “Ilinskij, Ilitshev”). Der Vorschlag das weiche Zeichen durch ein “’” wiederzugeben ist schon deshalb nicht annehmbar, da das “’” gewöhnlich das harte Zeichen ersetzt und diese Transkription nur eine unnötige Konfusion hervorrufen würde.

10. Was nun die Transkription des russischen “ѣ” betrifft, so wäre es wohl angebracht, es dem russischen “e” gleichzustellen, dass heisst also, es durch “e” oder durch “je” zu transkribieren (am Beginn der Worte und vor einem Vokal). Freilich, auch in manchem anderen Fällen besoders nach einem russischen “н” macht sich das “ѣ” gut bemerkbar und wird auch gewöhnlich durch “je” wiedergegeben, wie z.B. “Dnjepr”, “Dnjestr” — eine Schreibweise die sich bereits ziemlich eingebürgert hat. In anderen Fällen aber kommt die Jotierung nur wenig zum Vorschein und entspricht hier das “ѣ” dem “e”, wie z.B. “Medvedev” (anstatt “Medvjedev”). Der springende Punkt ist aber, dass das “ѣ” in der neuen russischen Grammatik gestrichen oder genauer gesagt durch das “e” ersetzt wurde. Es wäre daher anzunehmen, dass im Weiteren auch die meisten Russen nicht mehr wissen werden, welche Worte mit “ѣ” geschrieben wurden. Es erscheint daher am zweckmässigsten, wenigstens für unsere praktischen Zwecke, auf die schliesslich gering Aussprache-Unterschiede des “ѣ” zu verzichten und es dem russischen “e” gleich zu stellen.

11. Das russische “э” entspricht der Aussprache nach ziemlich genau dem lateinischen “e” und kann ohne Weiteres durch “e” wiedergegeben werden. Der Vorschlag es durch “eh” zu transkribieren ist unverständlich, da der dadurch entstehende Laut dem “э” nicht entspricht. Wenn wir das russische Wort “эхо” als “Echo” schreiben, so erscheint seine Leseart recht deutlich und der russischen Leseart hahe, dagegen “Ehcho” ist eine unnötige Komplizierung der Schreibweise, die dei Aussprache nur unnütz verwirrt.

12. Wegen dem russischen “і”, d.h. wegen seiner Gleichstellung dem russischen “и” und dementsprechend dem lateinischen “i” bin ich mit den Ansichten der genannten Autoren (*l.c.*, p. 16, Nr. 26) volkommen einverstanden.

III. Vergleichstabelle der Transkription der russischen Buchstaben laut meinen Vorschlag und dem von Almasov & Boltovskoy

Russisches Alphabet	Lateinische Transkription	
	Mein Vorschlag	Vorschlag von Almasov & Boltovskoy
a	a	a
б	b	b
в	v	v
г	g	g
д	d	d
e	e, je	e
ë	jo	e
ж	zh	zh
з	z	z
и	i	i
i	i	i
й	j	j
k	k	k
л	l	l
м	m	m
н	n	n

Russisches Alphabet	Lateinische Transkription	
	Mein Vorschlag	Vorschlag von Almasov & Boltovskoy
о	o	o
п	p	p
р	r	r
с	s, ss	s
т	t	t
у	u	u
ф	f	f
х	ch	kh
ц	tz	th
ч	tsh	ch
ш	sh	sh
щ	stsh	shch
ъ	'	w
ы	y	y
ь	j	'
ѣ	e, je	e
э	e	eh
ю	ju	ju
я	ja	ja

DOCUMENT 26/10

Comment on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

By **H. S. BUSHELL**

(*Commonwealth Institute of Entomology, London*)

(Letter dated 21st February 1956)

This scheme is proposed both for the needs of zoological nomenclature (as specified by the Commission) and for the more varied needs of bibliography, indexes, etc. It seems to me, however, that these two sets of needs are different in character, and that any scheme designed to satisfy both cannot completely satisfy either. I will, therefore, comment on the two aspects separately.

Bibliography and Indexing

The needs that a scheme for this purpose has to satisfy are set out on page 4 of the transliteration system published by the Royal Society in 1953. I may recall that, in order of importance, they are

(1) Avoid ambiguity completely even for those who do not know any of the languages from which transliteration is made (demonstrated by back-transliteration).

(2) Make names that can be indexed and can be found in an index.

(3) Do not use diacritics not available to English printers.

(4) Indicate pronunciation, so far as may be possible after requirements 1–3 have been fulfilled.

The Almasov-Boltovsky scheme fulfils the third of these requirements completely, but it does not fulfil the first, second or fourth. It does not fulfil the first, because many of its diagraphs (or other combinations of letters are made up of letters that are also used singly for other letters. Most (but not all) of these diagraphs include the letter j, and as regards the ones that do, the authors write (p. 15, para. 19) " such criticism would be justified if such double usage would be a handicap for ' inverted transliteration '. However, in all five Slav languages corresponding sounds are represented in an absolutely definite way, thus eliminating the possibility of confusion ". This means that there is no confusion for those who know the practices of the five languages, but complete confusion for those who do not, and the latter include the great majority of scientific workers in western Europe.

The scheme does not fulfil the second requirement (indexing) because it introduces a number of letters that are not to be pronounced but function only as " supplementary signs ". These letters would cause words to occupy unexpected positions in an index and hence be difficult to find. The scheme does not fulfil the fourth requirement (pronunciation) because some of its diagraphs and other combinations are obviously used to show pronunciation completely (e.g. sh [no. 37]), some show pronunciation in part (e.g. chj [no. 29]), some show it for one language but not another (shch [no. 38] is correct for Russian, but is sht in Bulgarian), and some do not show it at all (e.g. ew [no. 10] or th [no. 34]). The use of th for no. 34 is also unfortunate in a special way : no. 34 is a common Russian letter, pronounced ts as in " bits " or " tsetse "—it is the first letter of the word tsar. When the Royal Society's system was being prepared, its transliteration (and the avoidance of resulting ambiguity) was discussed with the Russians, the Yugoslav embassy and Sir Ellis Minns of the British Academy, and they all refused to accept any alternative to ts.

Zoological Nomenclature

The desiderata for the name of a genus or species appear to be that it should be latinised so as to be easy to print and pronounce and should not be made longer than need be. Back-transliteration (of " Cyrillic-derived " names) is not necessary and not possible. It is not necessary because, for example, one does not need to know in whose honour a species was named in order to find its original description. [What one does need to know is the name of its author, and this should therefore be transliterated by an unambiguous bibliographical system.] It is not possible, because the Commission does not propose any change in existing names that do not include diacritics, and as such names (e.g. *tschitscherini* or *latyschewi*) have been transliterated by a variety of systems, they could not be back-transliterated by any one system. It is not desirable to base zoological names on bibliographical transliteration systems, because those would make the names too complicated. The Royal Society's system would be unsuitable because it contains a few diacritics, and the scheme of Drs. Almasov and Boltovskoy would increase the length and complexity of many names and render them difficult to pronounce, because it would introduce into them additional letters intended to be mute and to act as supplementary signs. It may perhaps be pointed out that the ew in the *latyschewi* mentioned above is the German transliteration of the two letters that we represent by ev, whereas the ew of the scheme represents a single letter with w as a supplementary sign. A far simpler system could be devised for zoological nomenclature, provided that its use were strictly confined to this purpose (it would be disastrous for bibliography). In this connection, the Commission's proposals have to be considered under two heads :—

(1) **Names that have already been published.** Here the zoologist has to modify a name that is already in Latin letters, because some of them have diacritics. It is of little use to tell him what to do with Cyrillic letters, because it may not be possible to back-transliterate the name and so know what the Cyrillic letters were. He needs to be told what to do with the Latin letters that have diacritics ; the instruction should be something like " change č into ch, š into sh, etc.". The diacritic letters to consider are those given in the table on the last page of the Royal Society's system, but there would not be so many of them if the obsolete letters were omitted. Moreover the troublesome digraph šč (Russian) and št (Bulgarian) need not be dealt with at all, because the right alteration, to shch (Russian) and sht (Bulgarian), will result if the instructions for š and č as individual letters are followed.

(2) **Names to be made for the future.** A transliteration system is needed here, and it needs to be simpler than published ones. A simple one could be devised provided that it was agreed that possibility of back-transliteration is not a factor. It could be based primarily on the use of y with all the sounds it has in English, e.g. Yugoslavia (consonantal), Lysenko (vowel) and Boltovskoy (component of diphthong like English " boy ").

Finally it seems to me that the Commission must decide which alphabets it should cover before it decides details of transliteration. The modern alphabets in which the Cyrillic letters are used are Russian, Bulgarian, Serbian, Ukrainian, Belorussian and Macedonian. The Royal Society's system was restricted to the first three of these six, because it was thought that these were the only ones in which matter of scientific importance was likely to be published, and additional alphabets meant additional complexity. Drs. Almasov and Boltovskoy include all except Macedonian, but if scientific matter were to be published in any of the last three languages, it would perhaps be more likely, for political reasons, to be in Macedonian, which the Yugoslavs wish to encourage, than in Ukrainian or Belorussian, which Soviet Russia apparently does not. I do not, of course know which Cyrillic alphabets the Commission will wish to consider, but a further point that troubles me somewhat is whether it has considered the problem of the alphabets of the Slav countries that use Latin letters but have diacritics on or under some of them. This is of course not a problem of Cyrillic transliteration but solely one of diacritics,. The Slav-Latin alphabets are Croat, Slovenian, Czech, Slovak and Polish. The first two would present little difficulty, but the last three have a number of diacritics additional to, and more difficult than, those that are concerned in Cyrillic transliteration. I should think that it is much more likely that a species will be named after, and in the Latin alphabet of, a Czech or Pole than that it will be named after, and also in the transliterated alphabet of, a native of the Ukraine or Belorussia.

DOCUMENT 26/11

Comment on the Almasov/Boltovskoy Plan for the transliteration of Cyrillic Characters

By **P. J. M. GEELAN**

(*London*)

(a) Letter dated 20th November 1956

The Cyrillic alphabets most likely to be involved in zoological names are, I imagine, Russian, Bulgarian and Serbian. The systems recommended by my Committee for the transliteration of Russian and Bulgarian are identical with those used officially in the United States. These were developed principally with the idea of the romanization of geographical names in mind, and you will notice that they employ a minimum of diacritical marks (in practice the umlaut can be omitted from Russian ë in transliteration, leaving only Bulgarian ŭ, in which the short sign should be retained. Anglo-American official practice is also uniform in the treatment of Serbian : this is to transliterate it into the Croat roman alphabet, which has a one-to-one correspondence with Serbian Cyrillic. I would draw your attention here to the dangers of using any other method of transliterating Serbian ; for all practical purposes Serbo-Croat is a single language which may be written in either roman or Cyrillic letters. The existence of two roman forms of Serbian names can only lead to complete confusion.

With regard to your particular problem, one's first thought is that the International Congress of Zoological Nomenclature should consider adopting an internationally accepted system of Cyrillic transliteration, such as that recommended by the International Standards Organization (details of this are obtainable from the British Standards Institute), which deals with Ukrainian and White Russian as well as the three languages mentioned above.

The difficulty about the I.S.O. transliteration from your point of view, however, is that it involves extensive use of diacritical marks (č, š, ž, etc.). It would of course be possible to accept the I.S.O. system without its diacritics, but the omission of diacritics in transliterated scripts is not at all comparable with the ignoring of them in a roman alphabet, and I would strongly advise against this course.

The best chance of resolving your problem satisfactorily seems to me to lie in getting the Congress to accept for international zoological use one or other of the national transliteration systems of member countries. The choice would presumably lie between French, German and Anglo-American systems. Allowing that the desideratum is the least possible use of diacritical marks, then the flexibility of the English alphabet in general permits a more precise and less ambiguous romanization than does French or German.

(b) Letter dated 28th November 1956

I read Almasov's paper on the transliteration of Cyrillic with great interest. The system he proposes is of course open to many objections (to apply it to Serbian, for instance, would be dangerously misleading), but it demonstrates quite clearly the difficulty of finding one general scheme for transliterating all Cyrillic alphabets. However, given the three impossible requirements of clerical applicability to all Cyrillic alphabets, the non-use of diacritical marks, and reversibility (to the original), then it seems to me that the authors have produced what is probably as good a solution as could be devised.

DOCUMENT 26/12

The System for the transliteration of Cyrillic Characters recommended by the Royal Society in its publication entitled " The Transliteration of Russian, Serbian and Bulgarian for Bibliographical Purposes "

TABLE I

THE RECOMMENDED SYSTEM – CYRILLIC-ENGLISH

No.	Letter	Language	Transliteration	No.	Letter	Language	Transliteration
1	А а	R S B	a	24	С с	R S B	s
2	Б б	R S B	b	25	Т т	R S B	t
3	В в	R S B	v	(25+24)	ТС тс	R S B	t-s
4	Г г	R S B	g	26	Ћ ћ	S	t'
5	Д д	R S B	d	27	У у	R S B	u
6	Ђ ђ	S	j'	28	Ф ф	R S B	f
7	Е е	R S B	e	29	Х х	R S B	kh
8	Ё ё	R	ë	30	Ц ц	R S B	ts
9	Ж ж	R S B	zh	31	Ч ч	R S B	ch
10	З з	R S B	z	32	Џ џ	S	j
11	И и	R S B	i	33	Ш ш	R S B	sh
12	І і	R	ī	34	Щ щ	R	shch
13	Й й	R B	ĭ	35	Щ щ	B	sht
14	Ј ј	S	y	36	Ъ ъ	R	‘
15	К к	R S B	k	37	Ъ ъ	B	ŭ
16	Л л	R S B	l	38	Ы ы	R	ȳ
17	Љ љ	S	l'	39	Ь ь	R B	'
18	М м	R S B	m	40	Ѣ ѣ	R B	ê
19	Н н	R S B	n	41	Э э	R	é
20	Њ њ	S	n'	42	Ю ю	R B	yu
21	О о	R S B	o	43	Я я	R B	ya
22	П п	R S B	p	44	Ѳ ѳ	R	th
23	Р р	R S B	r	45	Ѵ ѵ	R	y̆
				46	Ѫ ѫ	B	ū

If a foreign name that has already been transliterated into Cyrillic letters has to be re-transliterated into English letters, the transliterated name should be followed by its original form in square brackets, e.g. Uittinkhem [Whittingham].

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[1] The tables given in the present Document are reproduced by kind permission of the Royal Society.

TABLE II

THE RECOMMENDED SYSTEM – ENGLISH-CYRILLIC

English Letters	Languages	Cyrillic Letters	English Letters	Languages	Cyrillic Letters
a	R S B	А а	p	R S B	П п
b	R S B	Б б	r	R S B	Р р
ch	R S B	Ч ч	s	R S B	С с
d	R S B	Д д	sh	R S B	Ш ш
e	R S B	Е е	shch	R	Щ щ
ë	R	Ё ё	sht	B	Щ щ
é	R	Э э	t	R S B	Т т
ê	R B	Ѣ ѣ	t'	R B	ТЬ ть
f	R S B	Ф ф	t'	S	Ћ ћ
g	R S B	Г г	th	R	Ѳ ѳ
i	R S B	И и	ts	R S B	Ц ц
ī	R	І і	t-s	R S B	ТС тс
ǐ	R B	Й й	u	R S B	У у
j	S	Џ џ	ŭ	B	Ъ ъ
j'	S	Ђ ђ	ū	B	Ѫ ѫ
k	R S B	К к	v	R S B	В в
kh	R S B	Х х	y	S	Ј ј
l	R S B	Л л	ȳ	R	Ы ы
l'	R B	ЛЬ ль	y̆	R	Ѵ ѵ
l'	S	Љ љ	ya	R B	Я я
m	R S B	М м	yu	R B	Ю ю
n	R S B	Н н	z	R S B	З з
n'	R B	НЬ нь	zh	R S B	Ж ж
n'	S	Њ њ	'	R	Ъ ъ
o	R S B	О о	'	R B	Ь ь

Notes.

The use of l', n' and t' in Russian and Bulgarian (where they are not transliterations of a single letter, but merely l, n or t followed by the soft sign) is included to emphasize the fact that they are used for single letters only in Serbian.

Shch and sht invariably represent the letter Щ in Russian and Bulgarian respectively, since the pairs of letters (ШЧ in Russian and ШТ in Bulgarian) seem never to occur consecutively in the respective languages.

TABLE III

THE SERBO-CROATIAN ALPHABETS LATIN (CROAT) AND CYRILLIC (SERBIAN)

Croat	Serbian	Croat	Serbian
A a	А а	L l	Л л
B b	Б б	Lj lj	Љ љ
C c	Ц ц	M m	М м
Č č	Ч ч	N n	Н н
Ć ć	Ћ ћ	Nj nj	Њ њ
D d	Д д	O o	О о
Dž dž	Џ џ	P p	П п
Đ đ (or Dj dj)	Ђ ђ	R r	Р р
E e	Е е	S s	С с
F f	Ф ф	Š š	Ш ш
G g	Г г	T t	Т т
H h	Х х	U u	У у
I i	И и	V v	В в
J j	Ј ј	Z z	З з
K k	К к	Ž ž	Ж ж

The order of letters in the Latin form of Serbo-Croatian is that given above; the order in the Cyrillic form is that given for the Serbian letters in Table I.

Amplifications of the Croat Alphabet.

Russian and/or Bulgarian letters that do not occur in Serbian Cyrillic are

ё, і, Й, щ, ъ, ы, ь, ѣ, э, ю, я, ѳ, ѵ, Ѫ.

Transliterations adopted for these in International Systems include:-

Cyrillic	Transliteration	Cyrillic	Transliteration
Ё ё	o, jo or ё	Ѣ ѣ	ě
І і	i or í	Э э	ė
Й й	j or ǐ	Ю ю	ju
Щ щ	šč (Russian) and št (Bulgarian)	Я я	ja
Ъ ъ	' or '' (Russian) and ă (Bulgarian)	Ѳ ѳ	f or ḟ
Ы ы	y	Ѵ ѵ	y̍ or ẏ
Ь ь	' or j	Ѫ ѫ	ą or ǫ

DOCUMENT 26/13

Letter dated 25th February 1958 from the Royal Society

(Reference : Document 26/12)

In 1953 the Royal Society and the British Academy drew up a system for the transliteration of Russian, Serbian and Bulgarian and this has been adopted by a number of scientific organizations in this country. At the time of drawing up this system there were, in use in the United Kingdom, a great number of systems of various kinds and it was in order to obtain some standardization among the scientific community that the Royal Society published its scheme.

Recently discussions have been held with the British Standards Institution and these are resulting in the publication by that Institution of a British Standard for the Transliteration of Cyrillic and Greek. This Standard will contain what, in effect, is the first agreed British system for transliteration of Cyrillic. This system is not yet published although it is hoped that it will be within the next two or three months. The transliteration of the Russian alphabet is based upon that of the Royal Society and I enclose a copy of the Royal Society system duly modified. As you will see the modifications concern only two characters. One of these is the hard sign in the Russian and the other is the obsolete letter ѳ which is not now in current use in Russia.

The Serbian transliteration has been altered to be in accordance with the Royal Society recommendations given in Table III of the pamphlet. When the Royal Society originally promoted its scheme there was a general feeling that a straight latinization of Serbian was to be recommended. However, since 1953 the Serbian alphabet has been more and more written in Yugoslavia in the form recommended in our Table III and in consequence the British Standard is recommending this kind of transliteration in order that confusion may be avoided.

As far as Bulgarian is concerned I am awaiting full details from the British Standards Institution and will let you have these as soon as possible.

Note by the Secretary to the Commission (intld. F.H. 28th February 1958):

The enclosure to the Royal Society's letter of 25th February 1958 is nor reproduced here because the Tables concerned are identical with those already given in Document 26/12, except for the insertion therein of the following modifications :—

(a) Table I, letter 36, last column : The single symbol ['] given in the original table has been replaced in the revised table by a double symbol [''].

(b) Table I, letter 44, last column : The transliteration "th" there suggested has been replaced by the symbol "ḟ".

(c) Table II : This is a Latin alphabet equivalent of Table I and the changes noted under (a) and (b) above have been included in it in the revised version now furnished by the Royal Society.

DOCUMENT 26/14

Transliteration system for Russian and Bulgarian geographical names prepared by the "Permanent Committee on Geographical Names for British Official Use"

(Communicated by the Permanent Committee)

TABLE 1

TRANSLITERATION OF BULGARIAN GEOGRAPHICAL NAMES

The following system for the transliteration of Bulgarian, devised by the Uited States Board on Geographic Names (BGN) and published by them in May, 1949, ws accepted for British official use by the Committee in September, 1952, and sould be referred to as the BGN/PCGN System.

Bulgarian	Transliteration	Bulgarian	Transliteration
А а	a	П п	p
Б б	b	Р р	r
В в	v	С с	s
Г г	g	Т т	t
Д д	d	У у	u
Е е	e	Ф ф	f
Ж ж	zh	Х х	kh
З з	z	Ц ц	ts
И и	i	Ч ч	ch
Й й	y	Ш ш	sh
К к	k	Щ щ	sht
Л л	l	Ъ ъ	ŭ
М м	m	ь	' (apostrophe)
Н н	n	Ю ю	yu
О о	o	Я я	ya

In transliteration from sources written in the orthography which was official before February, 1945:

1. Word-final ъ should be omitted in transliteration.
2. The obsolete letter ѫ , now replaced by ъ , should be transliterated by ŭ.
3. The obsolete letter Ѣ , replaced in February 1945 by E or Я according to local pronunciation, should be transliterated by ye, if sources written in the new orthography are not available. Names transliterated with ye resulting from Ѣ should be corrected to agree with the new Bulgarian spellings as they become available.

TABLE 2

TABLE FOR THE TRANSLITERATION OF RUSSIAN GEOGRAPHICAL NAMES

RUSSIAN	TRANSLITERATION	RUSSIAN	TRANSLITERATION
А а	a	Р р	r
Б б	b	С с	s
В в	v	Т т	t
Г г	g	У у	u
Д д	d	Ф ф	f
Е е	ye, e[1]	Х х	kh
Ж ж	zh	Ц ц	ts
З з	z	Ч ч	ch
И и	i	Ш ш	sh
Й й	y	Щ щ	shch
К к	k	Ъ ъ	”
Л л	l	Ы ы	y
М м	m	Ь ь	’
Н н	n	Э э	e
О о	o	Ю ю	yu
П п	p	Я я	ya

[1] ye initially, after vowels, and after ъ, ь; e elsewhere; when written as ë in Russian, transliterate as yë or ë.

DOCUMENT 1/51

(*continued from page* 350)

The Moore/Sylvester-Bradley " Parataxa Plan " : Note of Support supplementary to Document 1/43

(Commission Reference : Z.N.(S.) 1056)

By **JOHN S. HAMPTON**

(*Bromley, Kent, England*)

(Enclosure to a letter dated 7th December 1957)

(for Document 1/43 see 1958, *Bull. zool. Nomencl.* **15** : 245–246)

The Holothuroidea differ from all other Echinoderm classes in having a greatly reduced calcareous skeletal system. External plates are typically absent and usually a peripharyngeal crown, anal plates, madreporite, and sclerites are present. Of these the endoskeletal sclerites (or ossicles) formed in the superficial dermal layers constitute the outstanding character of the class, they are of microscopic size and occur in an endless variety of shapes ; an association of several types being usual in individual specimens. Their shapes are of paramount importance in the species identification of Recent forms, each of which is characterised by its sclerites (Hyman, 1955 : 134).

2. As explained in my note of 12th November 1957, dissociated fossil sclerites are the only basis for an understanding of the palaeontology of the class Holothuroidea. They are widely distributed in marine sedimentary strata, but are seldom common. The study of fossil sclerites, however, presents an almost completely unexplored field in micropalaeontology, as no method has been provided, other than the dual nomenclature outlined below, for applying names to them without reference to the whole-animals which they represent. The present classification of fossil Holothuroidea proposed by Frizzell & Exline (1955 : 56), is based completely on disjunct components (sclerites) and, although, as in any classificatory system based on form and structure, it unavoidably follows to some extent the pattern of natural (genetic) relationship, is completely artificial. The arrangement of families is based on the development of sclerites in Recent holothurians ; a family being erected to include all those sclerites of a general morphological type. Genera, in turn, are grouped on more restricted features, and similarly species are based ideally on suites of nearly identical sclerites (Latin binomial names being applied). Markedly unlike sclerites are found within Recent biospecies, and apparently identical sclerites are reported to exist in relatively unrelated forms. No rule can be given for recognising variable sclerites, unless coherent variation can be shown, of a single biospecies, as opposed to similar sclerites belonging to distinct forms, and in micropalaeontological studies ontogenetic stages in sclerite associations of a biospecies must be ignored as their relationships cannot be determined (see Frizzell & Exline, *op. cit.*). The relationship of disjunct fossil sclerite " species, genera and families ", to their respective biospecies is, therefore, usually obscure (Hampton, 1957*a*). It appears obvious, therefore, that fossil holothurian sclerites are best fitted only for " Parataxa ", Frizzell & Exline recognise the categories of their classification (1955) as " Parataxa " (Frizzell, 1957). Such recognition of fossil holothurian sclerites as " Parataxa " would make the study of these discrete fragments of considerable value in stratigraphic-micropalaeontological research.

3. In recent micropalaeontological studies (Hampton, 1957, *b*, *c*, *d* and *e*) several new " genera, subgenera and species " of holothurian sclerites have been proposed. The artificial nature of these categories was recognised, as was the need for a separate classification (Hampton, 1957*a*), and as they were proposed within the classification of Frizzell & Exline (1955), they are best considered as " Parataxa ".

4. It is for the foregoing reasons that, as I have said in my communication of 12th November 1957, I strongly support the proposals of Moore & Sylvester-Bradley (1957) to apply in the study of these discrete fossil fragments.

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